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CANTOR LECTURES

ON

INSTRUMENTS

FOR

MEASURING RADIANT HEAT.

BY

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SYLLABUS.

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INSTRUMENTS FOR MEASURING RADIANT HEAT.

BY

C. V. BOYS, A.R.S.M., F.R.S.

LECTURE I.—DELIVERED MARCH 25, 1886.

At the time that I was honoured by the invitation to give a course of Cantor lectures, I was so much absorbed in my experiments on the development of instruments for measuring radiant heat, that I naturally turned to that subject as one which I hoped would be worthy of being discussed in the rooms of the Society of Arts. However, now that the time has come when I have to put before you an account of the different instruments of this class that have been made, and of the principles upon which they depend, I feel, when I consider what splendid courses of Cantor lectures have been delivered in this room, how utterly unable I am to follow in the steps of those that have gone before me, or to treat my subject in the manner that it deserves.

When an ordinary candle burns, heat is developed, which escapes chiefly in the stream of hot air which rises from the flame. This stream is sufficiently powerful to keep a screw of paper at a height of six feet constantly rotating. If there are many candles or lamps, then the number of separate streams unite in a lake of hot air, which may be found resting under the ceiling of the room. Heat escaping in this way is said to escape by convection. If a piece of copper wire is placed with one end in the flame it also becomes hot, and some heat escapes along the wire, so that a ball supported by wax falls when the wax is melted, or a piece of phosphorus catches fire, or the fingers holding the wire are burnt. This passage of heat through a material which it warms is called conduction. Finally, if the finger is held a few inches away from the flame, and about level with it, though no hot air is driven in that direction, the finger clearly feels the sensation of heat. Heat is escaping in all directions from the flame without warming the air round about, and without being sensible until it falls upon some obstruction, when its existence becomes known. This heat escaping by radiation may be felt in any room in which a group of gas burners is alight. All that is necessary is to take a sheet of tin plate and hold it in such a position as to reflect the light—and therefore the heat —on to the face. If the plate is suitably bent by hand, not only will it be filled with light, but the heat which then falls on the face is evident at distances at which we might think some very delicate apparatus would be necessary to detect it. The heat felt under these conditions travels through air without appreciable absorption, just as light does. The air is not warmed in the process; the energy of the radiation passes on unchanged, and only becomes sensible as heat when it meets with some obstruction. In the case of the sun, no heat can escape by convection, for there is no atmosphere outside it in which currents can be produced. None can escape by conduction, for it does not rest upon anything. All the heat which reaches the earth from the sun, all which leaves the sun at all, is, as far as we know, due to radiation.

The relative amount of heat which escapes from hot bodies by the three processes which have been described — convection, conduction, and radiation — varies very widely but in general, except among the heavenly bodies, the first two between them are far more important factors in the cooling of a body than radiation. The amount of heat which escapes by radiation is freely radiated into all space, so that if the obstructing body is small, or is any considerable distance away, but a small proportion of the radiation falls upon it, the rest, of course, escaping in all directions. For these two reasons instruments which are intended to measure radiant heat, as it is commonly called—or radiant energy, which is a better term—must in general be capable of showing quantities of heat which

are very small in comparison with that stored or developed in the radiating body.

These instruments differ from thermometers, in, that when a thermometer gives a steady reading temperature is indicated, and there is no heat flowing to or from the thermometer. On the other hand, with instruments for measuring radiant heat, instruments that would be called radiometers if Mr. Crookes had not already given that term a special meaning, the actual temperature of the hot body is but one of the numerous factors which determines the indication of the instrument, and, further, the instrument only gives a steady indication when the rate at which it receives heat from the hot body is equal to the rate at which the part of the instrument heated by the radiation loses heat in consequence of its excess of temperature. At first the exposed part increases in temperature; as it is warmed it loses heat, generally, in all three ways, by conduction, convection, and radiation; this loss becomes faster as the temperature rises, and in time a steady state is arrived at, when the rate at which heat is received is equal to that at which it escapes.

Among instruments for measuring radiant heat I cannot do better than at once refer to the thermometer with a blackened bulb *in vacuo*. This instrument will of course, in time, show the temperature of any enclosure in which it may be placed; it then acts as a thermometer simply, and the vacuum round the bulb merely serves to make the process of acquiring the temperature of the enclosure slower than it would be if the intermediate space were filled with air; or it may be exposed to the sun's rays, in which case, if it did not lose heat at all, it would go on rising in temperature, slowly, possibly, but still without stopping until it acquired the temperature of the sun, or was destroyed in the process. In this instrument everything is done that can be done to reduce the loss of heat. Because the bulb is in a vacuum, convection of heat is prevented; because the bulb is only supported by a slender stem of glass, conduction of heat to the outer world is almost inappreciable. However, radiation remains, and it is this that determines the temperature to which the bulb will rise when exposed to any given source of radiation. Though the rate at which the bulb loses heat at any given temperature would be diminished by silvering it, such a silvered bulb would not become actually hotter, because it would absorb heat more slowly in about the same proportion. The chief advantage that is obtained by the use of the black surface is quickness, for both the gain and the loss go on at a higher rate than would be the case with any other surface, and, therefore, the final or steady state is more rapidly brought about. This instrument would more truly measure the relative heat of the sun's rays if it were fixed in a place having a constant temperature. When it is simply placed out of doors it is impossible to say what the temperature of the surrounding bodies—the ground, the walls, the sky—really is; but, on the whole, the rate at which heat leaves the bulb is greater in winter than in summer, for a given excess of temperature of the bulb, and, therefore, the temperature to which it will rise for a given rate of radiation is less in winter than in summer. I have referred to this instrument at this stage because it illustrates well the difference between a thermometer, as commonly used, and an instrument for measuring radiant heat. It serves both purposes. It shows the temperature of an enclosure when outside radiation is prevented. By its excess of temperature above that of surrounding bodies it shows at what rate heat is being poured into it from the sun or other radiating body.

All instruments when used to measure radiant heat have in the same way some exposed part or sensitive surface, which is heated to such a temperature that it loses at the same rate that it absorbs heat; but in almost all cases there is this difference—instruments for measuring radiant heat do not, as a rule, give any indication of the actual temperature of the sensitive surface, but only of the excess of the temperature of this over that of the rest of the instrument, and thus their indications are equally valuable whatever the actual temperature may be.

When heat falls upon a thing and warms it, the rise of temperature produces a variety of physical effects, some of which are made use of to determine the rise of temperature, and therefore the rate at which heat falls upon the surface, while most are not conveniently available for this purpose. In the first place, whether the thing is a solid, or, if hollow, it contains a liquid or a gas, the rise of temperature in almost every case produces an expansion which may be made apparent by mechanical or electrical means.

If the thing that is warmed consists of a junction of two metals, an electromotive force is set up, producing a current the strength of which may be made a measure of the increase of temperature.

Or, again, if the thing that is warmed consists simply of a strip of conducting material, its electric resistance is altered—increased generally but decreased in the case of carbon—by a rise of temperature. Any change in the resistance of one part of a circuit through which a current is sent from an external source will alter the strength of the current, and if, by means of a differential galvanometer or Wheatstone's bridge, the effect of the current is balanced, then a change in the resistance will upset the balance, and this disturbance can be made evident by a galvanometer.

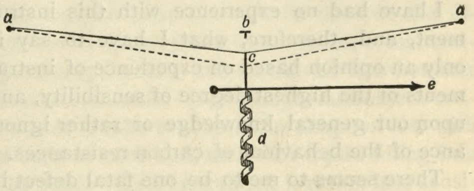
These three effects of heat—expansion, thermo-electromotive force, and change of resistance—are the only ones which are turned to account in instruments for measuring radiant heat.

Let us first consider the few instruments that exist, in which some material exposed to the radiant heat expands in consequence of the rise of temperature, and in which the change of volume gives rise to visible effects.

There are many ways of showing, with magnifying levers or other contrivance, that a rod of almost any material is lengthened by heat, but these are so insensitive that they would be quite unable to detect such feeble radiation as is easily measured in other ways. The expansion of a piece of brass wire, or even a glass rod, when slightly warmed, may be shown easily enough by allowing the end to roll over a fine needle, to which a straw is fastened as an index. Captain Cardew measures the rise of temperature in the wire in his voltmeter by magnifying the expansion with a wheel and pinion. The rise of temperature here, it is true, is not produced by radiation, but is the indirect result of the electromotive force to be measured; but I refer to it as it is one of the few practically useful instruments in which expansion produced by a rise of temperature in a solid is magnified and made evident by mechanical means.

Professors Ayrton and Perry magnify and measure the elongation due to the rise of temperature in the wire in their voltmeter by a very elegant device (Fig. 1). In the first place, there is a fine wire stretched between two fixed points, *a a*, and pulled on one side by the action of a spring, *d*. When the wire is warmed, the spring pulls it a little further to one side, because it is longer, to a position shown by the dotted line. Now the additional distance to which the wire is pulled to one side is greater than the increase in length of either half of the wire, in the same proportion that *a c* is longer than *b c*; *i.e.*, it would be longer in this proportion if it were not for the fact that, as the angle at *c* diminishes, the power of the spring to stretch the wire diminishes also; nevertheless, the lateral motion at *e* is itself considerably greater than the expansion of either half of the wire, and this increased motion is itself enormously magnified by the action of the spring itself. The spring is made with a twisted ribbon of the same shape as the shaving that is produced when a plane, held at an angle, takes a shaving off the edge of a plank. Such a spring (as Professors Ayrton and Perry have shown) twists through a large angle when it is pulled out, even through a small distance; therefore a balanced index on the end of the spring again magnifies the stretch of the wire, and thus a very feeble rise in temperature is made manifest.

FIG. 1.



I do not think that any one has suggested that these instruments might be used with advantage to measure radiation falling on the wire, nor do I think that any instrument depending on the expansion of a solid could compare in sensibility with those to be described.

The only instrument that I can remember that has been seriously put forward as a delicate means of measuring radiant heat, which does depend on the expansion which a rise in temperature produces in a solid body, is the tasimeter of Edison.

In this instrument the part of the apparatus exposed to radiation is a thin strip of vulcanite or of zinc, which is supported between a screw at one end and a carbon resistance at the other, so that when it expands by heat it shall increase the pressure on the carbon wafer, and so diminish its electrical resistance, an effect which can be easily and accurately observed by well-known electrical methods. There is a good account of this instrument in the *Telegraphic Journal* of November 15, 1878, by Professor Barrett, from which it is possible to get some idea of the sensibility of the tasimeter.

The following paragraph copied from the journal indicates what the instrument will do.

"The heat radiated from one finger held near the cone is more than sufficient to drive the galvanometer index right across, and off the scale. In a letter relating to this tasimeter, Mr. Edison writes to me as follows:—'By holding a lighted cigar several feet away I have thrown the light right off the scale,' and by increasing the delicacy of the galvanometer 'the tasimeter may be made so sensitive that the heat from your body while standing 8 feet from and in a line with the cone, will throw the light off the scale, and the radiation from a gas jet 100 feet away gives a sensible deflection.'"

Professor Barrett went on to say that he considered the tasimeter to be a more sensitive instrument than the thermopile, as a thermo-*scope*, but he did not think it would replace the thermopile, except possibly in investigations on the heat of spectra, owing to the linear form that can be given to the ebonite bar.

I have had no experience with this instrument, and, therefore, what I have to say is only an opinion based on experience of instruments of the highest degree of sensibility, and upon our general knowledge or rather ignorance of the behaviour of carbon resistances.

There seems to me to be one fatal defect in this instrument as an instrument of precision intended to be used in exact investigations. The indications of the instrument are perfectly arbitrary. We do not know the law according to which the resistance of these carbon wafers change with change of pressure. Though, as Professor Barrett says, as a thermoscope the instrument may be more sensitive than the ordinary thermopile, which at that time was the most delicate instrument in common use; though when nicely adjusted it might detect a minute change in the radiation falling upon it, the deflection of the galvanometer needle gave no measure of the amount of this change. But if the only fault was that the effect produced was not exactly proportional to the cause, a property common to many most useful instruments, this would be no great objection, for all that would have to be done would be to find out what deflections regulated additions of heat produced—it would be merely necessary to calibrate the instrument. But this I believe is impossible; I do not believe that if the instrument were set up and the calibration curves determined a dozen times, that any two of these curves would be the same. I simply state my belief; if I am wrong, I hope I may be corrected as speedily as possible. It is for this reason that I look upon this instrument simply as a -scope, and not a -meter.

Finally, among delicate means of observing the expansion of metal may be mentioned the well-known metallic thermometer of Breguet, which consists of three strips of metal—silver, gold, and platinum—soldered together face to face, and wound into a helix. During any change of temperature, the three metals change in length by different amounts, the platinum least and the silver most, and therefore the helix winds up or unwinds. This instrument, like the thermometer, essentially shows actual temperature, and not excess of temperature above surrounding bodies, a feature which does not belong to the voltmeters already mentioned, in which a change of temperature in the whole instrument produces no effect, for all the parts are made to expand alike.

The expansion of a solid might be practically employed in an instrument for measuring radiant heat solely because the solid can be rolled or drawn into very thin strips or wires, which quickly take up the final temperature. The expansion of liquids in tubes cannot conveniently be employed for this purpose, because the amount of liquid that would be sufficient to produce a visible effect would be so great that the time occupied in coming to the final temperature would be enormous. It is only in such cases as that already referred to, where a thermometer *in vacuo* is used to measure the sun's radiation, the changes in which are comparatively slow, that a liquid can be used with any advantage to measure radiant heat.

If the capacity for heat of the bulb of the thermometer is known, and the rate at which it rises in temperature when exposed to the sun's rays, and the average falling in temperature when the sun's rays are screened from the instrument, then, knowing the area exposed by the bulb, we have at once the means of determining absolutely the rate at which heat is reaching the surface of the earth. This measurement is, however, more accurately made by a class of instruments devised for the purpose, of which one of the best known is the pyrheliometer of Pouillet. This consists of a thin box of metal with a flat base; the box is filled with water, and there is immersed in the water the bulb of a delicate thermometer, the stem of which passes down the tube of the instrument. At the lower end of this tube is fixed a disc the same size as the base of the box, so that the observer can, by casting the shadow of the box on this disc,

place the bottom of the box square to the rays from the sun. As before, if the capacity for heat of the box, its rate of heating in the sun, and its average rate of cooling when shaded are determined, we have at once a measure of the rate at which we receive heat from the sun.

Improvements in these instruments have been made by placing the bulb of a thermometer in a cavity maintained at a constant temperature, so that the rate of cooling may be more regular; but as my object in these lectures is to deal rather with instruments of a far more delicate order, I shall not say more on this part of the subject. But perhaps it may be worth while to give the figures that have been obtained for the sun's radiation. The quantity of heat which reaches the earth yearly would be sufficient to melt a layer of ice about 100 feet thick spread over the surface of the earth, and the quantity leaving the sun by radiation is sufficient to melt every hour about 2,000 feet of ice over the whole surface of the sun.

When we make use of the expansion of a gas, we at once have the means of observing far feebler effects than are possible with liquids and solids; in fact, it is possible to produce an instrument in which the expansion of a gas indirectly shows the presence of radiation which rivals any of the modern thermo-electric apparatus in delicacy, though not perhaps in ease of application.

The first advantages gained by the use of gas is the small weight and the great rate of expansion with rise of temperature. Air expands far more than alcohol; at ordinary temperatures it has a specific heat far less than that of alcohol, and its density is $\frac{1}{700}$ of the density of alcohol. Thus a given quantity of heat applied to a certain volume of air would produce an expansion for each of these reasons greater than it would do if applied to the same volume of alcohol; but as the capacity for heat of the containing bulb, in the case of air, is everything; while in the case of alcohol it is practically nothing, the differences are not quite so great as would at first appear. Still, in spite of the weight of the thin glass bulb, the rate at which the bulb of air is heated is enormous compared with that at which the same bulb of alcohol would be heated, and so it comes to its final temperature far more quickly than would any liquid, and then when it has done so, the expansion is far greater. The expansion of air at constant pressure is most easily found by using a special scale of temperature obtained by adding the figure 273 to the temperature, as measured by a centigrade thermometer, and calling this the absolute temperature, then the volume is proportioned to the absolute temperature, if the pressure is constant, or the pressure is proportional to the absolute temperature if the volume is constant. Thus if we make an air thermometer with a bulb two inches in diameter, and a tube one-tenth of an inch in diameter in the bore, the bulb has a capacity of 533 inches of the tube, and therefore the degrees will be very nearly two inches long. The air thermometer, consisting simply of a bulb and an open stem containing an index of some liquid is a very inconvenient instrument, because every change in the pressure as shown by a barometer will produce its own effect; thus, if in the case taken the barometer were to fall one inch, from 30 to 29, the index would move more than 18 inches from this cause alone. For this reason, and also because in measuring radiant heat we want to observe the increase in temperature, and not the actual temperature, Leslie's differential air thermometer is more suitable. This simple instrument consists of a tube bent into the shape of a U with a bulb at each end, and with liquid filling half the tube. This instrument is rather less sensitive than the simple air thermometer, because in working it sets up a pressure due to the difference of level of the liquid in the two limbs which tends to compress the air where it is expanded, and to make it expand where it is contracted; further, the bulb not exposed to the radiation has the air in it compressed by the expansion of the air in the warmer bulb. The first of these opposing causes is removed in Rumford's differential thermometer, in which the horizontal limb of the U is very long, and the vertical legs short. The short index of liquid in moving along the horizontal tube does not set up any opposing hydrostatic pressure. With the first of these instruments Leslie made his researches on radiant heat before the thermopile had been invented, and as is so often the case with the true experimentalist, he thus made his most famous discoveries with the simplest possible means.

A modification of the differential thermometer has been devised by Prof. H. F. Weber, of Zurich, of which a very short account is given in the "Archives de Genève," 1887, p. 347; so short, in fact, that it is impossible to criticise the instrument until more details have been published.

In this instrument, which Professor Weber

calls a microradiometer, the two bulbs of a differential thermometer are replaced by two thin boxes of brass, one end of each of which is made of a plate of rock salt. These boxes are joined to the two ends of a glass tube with a bore about 1 square mm. in area, which has a small bulb blown near each end. The middle of this tube is filled with mercury, and the bulb and about 5 mm. of the tube at each end is filled with a solution of sulphate of zinc, which is prevented by capillarity from escaping into the boxes. If one box is warmed more than the other, then, as in the ordinary differential thermometer, the liquid in the tube is driven a small way towards the other bulb. The peculiarity of the instrument depends on the way in which this motion is made evident. The 5 mm. or so of sulphate of zinc solution between the bulbs, and the mercury in the tube at each end, form two of the arms of a Wheatstone's bridge; the other two arms consist of a pair of resistances as usual, which are put into electrical connection with the sulphate of zinc solution by wires sealed into the bulb. When, owing to the warmth of one of the boxes, the column of sulphate of zinc is lengthened in one end of the tube and is shortened in the other end, the resistance of the one end is increased and that of the other diminished, and thus there is a double disturbance of the balance of the bridge, which at once makes itself felt in the galvanometer.

With this complex apparatus Professor Weber says he can detect a one-hundred millionth of a degree, and that the heat of the moon produces an oscillation of about a hundred divisions of the scale.

I can only conclude, from the very short account at present published, that this instrument is very sensitive, far more sensitive than anyone would expect; but whether the indications given by the instrument bear any direct relation to the increase of temperature of the boxes, there is at present no evidence to show. The inventor gives as the theory of the instrument that it is a simple Wheatstone bridge, and that the change of resistance of the arms is the cause of the want of balance. I am inclined to think that this must be very much involved with another action that must certainly come into play. When one of the boxes is warmed, the liquid is driven from that side, but if the increase of temperature—and therefore the acting pressure—is very small, the liquid will not simply move along and take a new position; the ends of the column of mercury will certainly move irregularly, and will also change in shape to a slight extent. Owing to the change of shape electro-capillary action will be set up—that is, an electromotive force will be set up independently of that due to the battery, which will affect the galvanometer if the bridge connections are so made that the galvanometer is connected with the two bulbs; whereas it should produce no effect—or but a slight effect—on the deflection if the galvanometer connects the mercury thread and the two resistances. Which arrangement is made, the paper does not show. It is very difficult to believe that each hundred millionth of a degree by which one box is warmed will produce the proper motion of the liquid due to it. The pressure that this temperature represents is about one thirty thousand millionth of an atmosphere; that is a pressure of one thousand millionth of an inch of mercury, or a pressure of about one hundred millionth of an inch of water. The experience of most physicists is, that in a capillary tube with four separate capillary surfaces, such a pressure would not cause any real movement of the liquid as a whole. It is easy to understand that in the case of larger differences of temperature the effect produced will be so enormous that, if all went in proportion, the one-hundred millionth of a degree could be detected; just as in the ordinary wheel barometer—in which the motion of the mercury is greatly magnified—the index would be capable of showing, if all went in proportion, thousandths or perhaps ten-thousandths of an inch, whereas every one knows that in this case you can, by judicious tapping, make the index rest in a variety of positions. I do not wish to be understood to say that this is the case with Weber's instrument, but only that this is what any one would expect. It is to be hoped, therefore, that we shall have details before long which will set these doubts at rest.

The air thermometer referred to so far may be classed among statical instruments. They come to rest when a certain definite change of temperature has been produced, and the position of the index is a measure of that change or difference of temperature.

There is another way in which the expansion of a gas may serve to indicate change of temperature. When a gas—air, for instance—is warmed, and therefore caused to occupy a larger space, the density of the gas of course becomes less. The gas, being lighter, rises, and produces the well-known rising current of hot air to which I drew your attention at the beginning of this lecture. These air currents are only too well

known to the experimentalist. If, in weighing anything—a crucible, say—the thing being weighed is not quite cold, it warms the air round it, and sets up a current of air which altogether disturbs the balance, and makes the thing seem too light. If the sun shines upon one end of the balance case it warms it, and sets up air currents which again disturb the equilibrium. A gas burner near the balance will do the same thing. Cavendish found, in that famous experiment by means of which he found the mass of the earth, *i.e.*, the number of tons of material which go to make the earth, that a difference of temperature produced by definite means, but still probably too small to be shown with a thermometer, produced disturbances which altogether masked the effect which he was measuring, and these disturbances were simply caused by air currents. The Rev. A. Bennet, F.R.S., in a paper of very great interest, and which is very refreshing in these days of centimetres, grammes, and seconds ("R. Soc Trans.," 1792), shows how air currents set up in this way may be used to detect the most extraordinarily feeble radiation. I give his own account of his fifth experiment.

"Several other light substances were suspended by fine spider threads and placed in a cylindrical glass about two inches in diameter, as the thinnest part of the wing of a dragon-fly, thistle-down, and the down of dandelion; of these, the last appeared most sensible to the influence of heat, for when the down was fastened to one end of a fine gold wire it would turn towards any person who approached at the distance of three feet, and would move so rapidly towards wires only heated by my hand, as very much to resemble magnetic attraction."

Again, Mr. Crookes in some of his researches, when great accuracy was required, was obliged to place his balance in a vacuum, and then, curiously enough, other irregularities were observed which led him to examine in greater detail what happened when radiation fell on light suspended bodies. He found, as had been found before, that these things appeared to be attracted by the influence of radiation. They were really warmed, and the current produced caused the appearance of attraction. On trying the same experiment under diminished pressures, this apparent attraction after a time ceased, and then when the vacuum was sufficient, repulsion was observed instead. In this way he was led to the invention of that marvellous instrument, the radiometer, and afterwards to the discovery of those extraordinary effects due to what he called radiant matter. I do not know whether I ought not to include the radiometer and several other of Mr. Crookes's pieces of apparatus in the catalogue of instruments for measuring radiant heat. They certainly do measure it in a way; in fact, in one of his tubes Mr. Crookes arranged a torsion balance, by means of which he actually weighed the repulsion due to a certain beam of light. The subject of his instruments is itself so vast that I really dare not enter upon it, and I think I am justified, for radiometers do not, as far as I know, supply convenient means of making accurate comparisons of feeble degrees of radiation.

There is no end of the number of instances that might be given of the effect of air-currents produced by even feeble degrees of radiation. Every experimentalist must have met with many instances. Though the effects are so strongly marked, I do not think much has been done to make use of these currents to measure radiation. There are the experiments of Mr. Bennet and Mr. Crookes, already referred to; there is also an instrument devised by Joule, and described in the first volume of his papers published by the Physical Society, p. 535, which I cannot do better than describe in his own words:—

"A glass vessel in the shape of a tube, 2 feet long and 4 inches in diameter, was divided longitudinally by a blackened pasteboard diaphragm, leaving spaces at the top and bottom each a little over 1 inch. In the top space, a bit of magnetised sewing-needle, furnished with a glass index, is suspended by a single filament of silk. It is evident that the arrangement is similar to that of a 'bratticed' coal-pit shaft, and the slightest excess of temperature on one side over that on the other must occasion a circulation of air, which will ascend on the heated side, and after passing the fine glass index, descend on the other side. It is also evident that the sensibility of the instrument may be increased to any required extent, by diminishing the directive force of the magnetic needle." *

I have made and now show an instrument of this class, in which the vane consists of a fragment of straw suspended by a quartz fibre one ten-thousandth of an inch in diameter. The arrangement is so delicate that it is in this form quite unusable, but it would seem, as it did to Joule, to be capable of being developed into a serviceable instrument.

Both Joule and Weber make use of the

* "Proc. Manchester Lit. and Phil. Soc." Vol. iii., p. 73.

moon, to show the extreme sensibility of their instrument. It was long a question whether any heat was sent to the earth from the moon. Prof. Tyndall, in speaking of this heat, says:—"Concentrated by a polyzonal lens more than a yard in diameter upon the face of his pile, it required all Melloni's acuteness to *nurse* the calorific action of the moon up to a measurable quantity." This, however, on account of the absorption by the lens, is not really a fair comparison; but it is, I hope, sufficiently evident that, by means of the air current itself, it is possible to detect effects of heat so feeble in amount that nothing but the most delicate apparatus would be capable of making their existence evident.

LECTURE II.—Delivered April 1, 1889.

In the last lecture I dealt with the subject of instruments which show the effect of heat by the expansion of something, whether a solid, a liquid or a gas, and showed that while in general these instruments are not capable of showing very feeble effects, yet three of them—Edison's tasimeter, Weber's microradiometer, and Joule's convection thermometer—are, according to all accounts, much more sensitive to the influence of radiant heat than we should be led to expect if we consider the very small co-efficient of expansion of bodies for heat. In two of these instruments a change of electrical resistance is the indirect result of the increased temperature of the part of the apparatus exposed to radiation, and in these cases the sensibility simply depends upon the sensibility of the galvanometer that is used in conjunction with the apparatus.

The second class of instruments to which I referred in the last lecture, instruments depending on thermo-electromotive force, are of two kinds, those in which there is a fixed thermo-pile or thermo-junction, which sends a current when it is warmed round the needles of a galvanometer, the deflection of which is a measure of the heat poured into the instrument; and, secondly, those in which the thermo-electric circuit is suspended in a strong magnetic field, in which it tends to turn round when a current is sent round the circuit in consequence of the heating of the junction.

The third class of instruments are those in which heat changes the electrical resistance of a conductor which is exposed to the radiation, and in which the change of resistance is observed — as in the case of the tasimeter—by means of a galvanometer, either arranged differentially or with a Wheatstone's bridge, so as to obtain the greatest delicacy possible.

All these instruments for measuring radiant heat, which are of an electrical character, require the aid of a galvanometer, or in a few instances they act as their own galvanometer; but in every case they require that a suspending fibre of some kind shall be used, since no pivots that can be made are so devoid of friction as not to completely destroy all chance of observing those minute effects which are within the grasp of the experimentalist.

Since this subject of suspending fibres is of the first importance in nearly all instruments of precision, not only in those which form the subject of these lectures, and since the fibres that are at present in common use all are subject to defects of a very serious character, I feel that I shall not be giving too much time to this part of the subject if I devote the whole of this lecture to its consideration, more espe-

cially as I shall be able to show that the annoyance to which the physicist is constantly exposed, owing to the vagaries of his silk suspensions, is no longer a necessity, and that the limit of accuracy which in many cases is due to the same cause, no longer is determined by the uncertainty of the suspending fibre, but by other causes which, in the presence of silk, are of no account. I shall, therefore, treat this subject of suspension on rather broader lines than I would do if I were merely considering galvanometer construction.

Of all the means that we possess of measuring minute forces, the torsion of a wire or thread is more convenient than any other, because no matter whether the force to be measured is a mere pull, as in the case of an attraction, or a twist, as in the case of a magnetometer or electrometer needle, torsion may be used, as in the first case, the pull may be applied to the end of an arm carried by the torsion thread, and in the second the turning force or moment may be applied direct.

By means of the torsion of an ordinary piece of brass or German silver wire, a foot long, and as much as 1-50th of an inch in diameter, forces as little as the weight of a single grain can be made evident to an audience. For instance, I have a straw suspended from such a wire, on the end of which I have placed a fragment of sheet iron weighing 10 grains. A weak magnet is incapable of lifting the piece of iron, yet it is able to pull it round through a large angle, and therefore to twist the wire to the same extent. One-tenth of this movement of the straw would be visible to everyone in the room, and, therefore, a force as small as the weight of a single grain, a force which, to our senses is inappreciable, can be made evident with wire as thick as this. If, instead of a plain straw turning round over the table we had a fine index passing over a scale, or, better, a mirror reflecting a beam of light on to a scale, then it would be possible, with the same wire to observe the effect of a force a hundred or a thousand times less.

With a finer wire, as is evident, feebler forces may be measured, but it is not at once evident to what extent a fine wire, say one half the diameter, will be more sensitive.

If the wire has half the diameter it will be sixteen times as sensitive, because the torsion varies as the fourth power of the diameter.

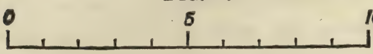
Now going back to the experiment in which it was shown that with a wire one-fiftieth of an inch thick the torsion was sufficiently small to make it possible to measure forces which to our senses are quite unobservable, forces of one-hundredth or one-thousandth the weight of a grain, it is evident what an enormously-increased delicacy would be obtained by using a wire, say, of one-tenth the diameter—and still finer wire than this can be obtained in any quantity. In this case, since $10 \times 10 = 100$, and $100 \times 100 = 10{,}000$, the force that would be required to produce a given twist in the thinner wire would be 10,000 times less than is necessary in the thick wire; therefore forces as small as one millionth or one ten-millionth of the weight of a grain would with this simple apparatus be observable.

It was with apparatus such as this, devised in the first instance by Mitchell, an English clergyman, that Coulomb made his famous researches upon the forces between electrified or magnetised bodies, and that Cavendish determined the mass of the earth.

The torsion of such a wire is so small that it might seem at first as if nothing finer could be required; but as a matter of fact, if the needles of an ordinary reflecting galvanometer were suspended by such wire, they would be held so immovably as to make them incapable of responding to such currents as are now easily measured. The forces and moments about which I have been speaking, though they seem very small indeed, are in reality enormous compared with those which can be easily measured. If still finer torsion threads are required a difficulty is met with. When wire is made much less than one five-hundredth of an inch, it begins to suffer in the process of drawing. Copper wire is now made almost one thousandth of an inch in diameter, but this wire is very weak, and difficult to use. I have a specimen of silver wire of this size, given me by Mr. Lecky, which shows well the way in which the surface has been damaged in the process of drawing. As there is a difficulty in showing these fine wires and threads to a number by means of the projection microscope, I have asked Mr. Chapman to photograph for me, with a microscope, the series of wires and threads of which I have to speak to-night, all on the same scale. Photographs perfectly show the appearance that these things present under the microscope, and at the same time they serve most excellently to show the actual size of the several fibres. In the first place, there is a photograph of

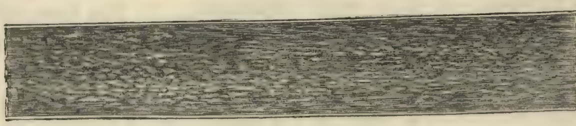
micrometer (Fig. 2).* Each division is one thousandth of an inch. I have made corres-

FIG. 2.



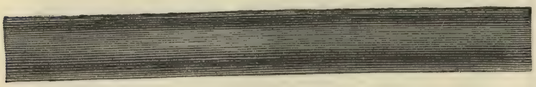
ponding marks on the screen, which will serve as reference marks when other slides are projected upon it. By way of giving some idea of the dimensions, I would point out that this wire to which I have so often referred would, on the scale on which the photographs are taken, completely cover the screen; it would appear about 25 feet wide. The next slide (Fig. 3) is an ordinary human hair, which I show because people speak of a hair as though

FIG. 3.



it were very fine. The next slide (Fig. 4) is a piece of copper wire, about one-thousandth of an inch in diameter.

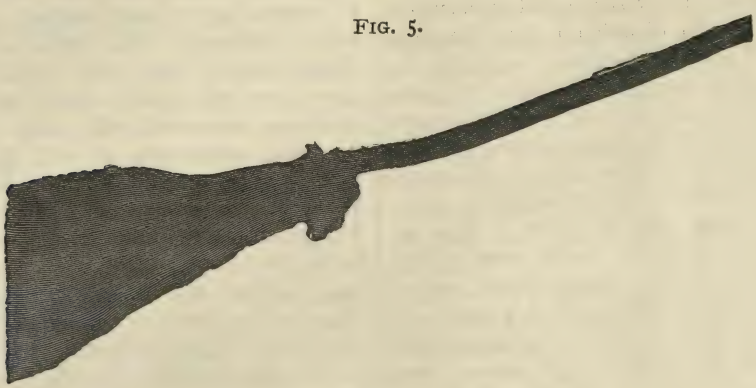
FIG. 4.



Very fine wire is made by Wollaston's process of enveloping a platinum wire in silver, and then drawing the compound rod into fine wire, after which the silver is dissolved by nitric acid. There is then left an exceedingly fine wire of platinum which, however, is, I believe, useless as a torsion fibre. The photograph on the screen (Fig. 5) is taken from a specimen from which the silver had been dissolved only up to a certain place. The end of the silver and the platinum core are evident. This is a two-thousandth of an inch in diameter; it is about the same size as a split cocoon fibre.

There is a beautiful material to which I imagine many experimentalists must have turned in their endeavour to find a fine

FIG. 5.



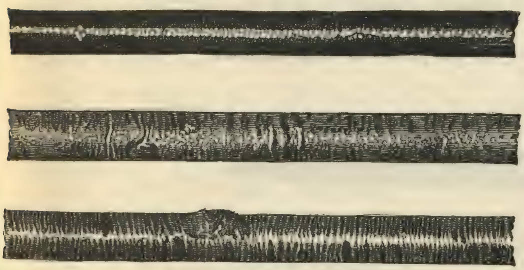
torsion thread. I mean this soft, silky-looking material—spun glass. The photograph (Fig. 6, p. 11) shows it to be fairly regular, and to be one-thousandth of an inch in diameter. This spun glass is a remarkable material in many ways. Experiments made in the physical laboratory at South Kensington by Messrs. Gibson and Gregory, show that it is stronger than it ought to be if its diameter only is taken into account. Their results are expressed in dynes per square centimetre, but for purposes of illustration, and because we are familiar with the strength of metals expressed in tons to the square inch, I have put their results in this form. Glass fibres about one thousandth of an inch in diameter have a strength of about 27 tons to the inch. The same glass in rods from 20 to 50 times as thick is from one-third to one-seventh the strength.

Thus it appears that though glass itself will not compare with iron in strength, spun glass

* The blocks of Fig. 1-7 have been kindly lent by the editor of "Nature."

has about the same strength as iron. On the other hand, while fine metallic wires are also stronger than they should be, I believe that when they are made as fine as possible the strength does not go on rising, but ultimately falls away again. Thus it is that spun glass is—or rather was until lately—the strongest thing of its size obtainable. This property of great strength is very valuable, because it makes it possible to suspend larger apparatus, and therefore to obtain larger effects than would be possible with a weaker material; or what comes to much the same thing, a finer fibre will carry a certain apparatus, and therefore there will be a greatly diminished torsion and increased sensibility. Other points in favour of spun glass are its uniformity, its freedom from atmospheric influence, and the fact that pieces of almost any length may be obtained.

Fig. 6.



There is, however, one very serious defect, which is so pronounced as to make this otherwise splendid material absolutely useless in instruments of precision. Suppose that the thing suspended carries a mirror, by means of which the deflection can be accurately observed. Now it is found that when all has come to rest, if a large deflection is produced by any means, then the zero is changed; the index comes to rest in a new place, and this place is not constant but slowly approaches the old position of rest. In some experiments which I made two years ago, I found this change of zero to be about 1-430th of the deflection if the mirror were kept deflected for one minute. Now the fact that the zero is changed every time the fibre is twisted, and that after deflection it is itself variable in position, makes spun glass a useless material for exact work. I may, however, say that I have found that this defect may be partially cured, if the angles of deflection are not large, by annealing the glass in a box of charcoal at a low red heat. In the case of a fibre nine inches long, and turned one complete turn, I found the change of zero after annealing to be about one-sixth of what it was before annealing.

It is partly for this reason, and partly because the torsion of spun glass is still so great, though it is only about one-third of what a brass wire of the same size would have been, if such fine wire were obtainable, that spun glass is never used in the construction of instruments. There was until lately no means of producing torsion threads finer than spun glass, and therefore the method of using torsion has been abandoned in all the more delicate apparatus, and fibres without torsion have been looked for which might be used to suspend the movable part of the instrument, while some subsidiary means is made use of to direct and give stability to its movement.

The most perfect torsionless fibre that I know of is the spider line. In the paper by the Rev. A. Bennett, to which I referred last week, there is an account of his experiments with spider web. He found that if he magnetised a sewing needle, and suspended it by a piece of spider web 2 inches long, he could turn the upper end of the web round 800 times, and yet the needle was not deflected to a visible extent. By way of making a more delicate experiment, he heated a piece of fine harpsichord wire in a candle flame, and allowed it to cool in the magnetic meridian, so as to obtain a very weak magnet. This hung at the end of a web 3 inches long, was not moved to a visible extent when the upper end was turned 1,000 times. Finally, he attached one fibre of the feather of a goose quill to a piece of web $2\frac{1}{2}$ inches long, and then turned it by a spinning machine more than 1,100 times, but the fibre did not go round. The thread became one inch shorter.

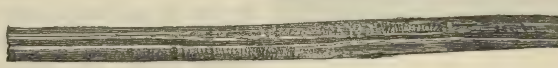
I do not think that much use has been made of spider line, except for cross wires in telescopes, and this is remarkable, for the very elaborate investigations of Joule* show it to be a trustworthy material. Joule experimented on the threads of the diadema spider, which are especially strong. Bennett does not specify the kind of spider that he went to for his threads. The strength of the diadema threads must be very great; Joule found that they were able to carry from 24 to 27 grains before breaking, and that they would carry for an indefinite time a load of 10 grains. He also found that with change of temperature the length of the loaded thread changed also, becoming shorter when warmed and longer when cooled. It also increased in length in a moist air, and contracted again in

* Joule's "Scientific Papers," vol., i., p. 479.

a dry air. However, the amount of this change is not very great. In a dry air a thread 23 inches long, and carrying 10 grains, would not change in length more than one-sixth of an inch, on an average, for a change of temperature of 100° Fahr. In six months it did not increase by one thousandth of its length. When the dry air was changed for wet air, the filament at once became half an inch longer. The length of diadema web does not seem to be subject to more uncertainty than the length of silk under the same conditions. If this diadema thread is as free from torsion as the spider thread used by Mr. Bennett, it would seem to be a perfect material for use in exact instruments. Why it is never* used I am unable to say. Joule speaks as though he used it in his arrangement of dip-circle, in which the needle is slung upon two loops of filament; but he says in the description of this instrument that he used silk.

Silk, as produced by the silkworm, consists of a pair of threads gummed together, about a two-thousandth of an inch in diameter apiece (Fig. 7). The silk must be washed in hot water to remove the gum, and the two fibres separated. The single fibre, according to A. Gray,* will carry a weight of about three or four grammes before it breaks, and will safely carry needles weighing from half to one gramme. The strength is approximately 340×10^7 dynes to the square cm., or 22 tons to the square inch. Though finer than spun glass, it is not quite so strong; it has a strength approaching that of ordinary iron. Silk has until lately been the material always used in instruments of the highest degree of precision. It has many valuable properties, but it is worse than glass in some respects. The torsion is so small that it is quite a usual thing to ignore it, though of course this should not be done in accurate experiments. Gray has given the results of his measures on the torsion of silk. Owing to the irregularity in the diameter, and the fact that it is not round, the figures are subject to a good deal of variation; however, they are worth giving, as they will be useful for purposes of comparison.

FIG. 7.



He finds that fibres from ·0009 to ·0015 centimetre in diameter have a moment of torsion of from ·00091 to ·0025 dyne-centimetre units. These measures give as the coefficient of torsion of the silk material numbers ranging from $1{\cdot}39 \times 10^9$ to $\cdot 495 \times 10^9$; or a rigidity of $\cdot 885 \times 10^9$ to $\cdot 316 \times 10^9$ when expressed in C.G S. units. The rigidity of spun glass, found by Messrs. Gibson and Gregory as the mean of a large number of experiments, is 205×10^9; so that it would appear that glass is from 230 to 650 times as rigid a material as the substance of which silk is made. Joule found that silk behaves much in the same way as spider thread when subject to heat and moisture.

The figures I have given are sufficient to show that the torsion of silk must be very small. Since it has a diameter about half that of spun glass, the torsion on this account would be one-sixteenth; and since the rigidity is so much less as well, the actual torsion is from 4,000 to 10,000 times less. We have already seen that the torsion of spun glass itself is so small that very feeble forces can be measured with it, but the torsion of silk is many thousand times less. No wonder that, since it is also as strong as bar iron, silk is nearly universally used.

It is, or rather was, universally used, because there was nothing better; good as its properties are, minute as its torsion is, I do not know any one who is altogether satisfied with it. The figure which represents its rigidity is true enough, and yet it is hopelessly delusive. Though the torsion, actually, is small, it is not constant, every variation of temperature or of moisture sets some new power in action so that the position of rest is never the same. This cannot be corrected by increasing the length of the fibre, because the uncertainty of the position of rest may be increased in the same proportion. Finally there is the same kind of elastic fatigue that I have described as making spun glass useless, and in a far higher degree, but the actual disturbing force here is so small that it is sometimes overlooked, though it absolutely prevents any galvanometer from

* Mr. Bottomley has told me since these lectures were delivered that he has found the spider line invaluable for suspending the mirror of a very delicate galvanometer.

* "Absolute Measurements in Electricity and Magnetism."

approaching the accuracy that would be obtainable with a perfect fibre.

Mr. Bosanquet, in a short article* which attracted a good deal of attention at the time, says:—

"At certain times the needle of the galvanometer would move about with sudden and capricious movements, the mirror often traversing several degrees of the scale. The decision and sharpness of the movements were very remarkable, and we habitually spoke of their cause as the 'ghost.' The ghost used to visit us mostly in summer, between the hours of nine and eleven in the forenoon, and six in the evening; when these movements began it was no use attempting to work with the galvanometer. There can be no doubt that the movements were due to the solar heat falling more or less directly on the instrument, and causing hygroscopic changes in the silk fibre."

He concludes the article as follows:—

"It is my conviction that silk and thread suspensions are sources of error and inconvenience to an extent that has been imperfectly realised; and that they ought to be entirely banished from all instruments of precision."

Mr. T. Gray,† who advocated the use of silk, while admitting that silk is not perfect, employed language which is hardly as strong as the subject deserves. He said:—

"When a galvanometer is made sufficiently sensitive for the fibre to play an important part in directing the needle, the set of the fibre, due to continued deflection, always produces an apparent change of zero which, in exact measurements, it is somewhat difficult to properly allow for."

Mr. Gray went on to say that the error is small, except in very special cases, and is in no way capricious. I believe I express the general feeling among experimentalists when I say that the behaviour of silk under ordinary circumstances appears, whether it really is so or not, extremely capricious.

Now, having insisted on the defects of the nearly universally-employed material, silk, I will next propose the remedy which I described before the Physical Society,‡ and which my later experience, confirmed by that of others, shows to be a remedy which is complete.

In my experiments on the development of the radio-micrometer, of which I shall speak this day fortnight, I required a torsion fibre finer than spun glass and free from the vagaries of silk. I required a thread with a torsion not much greater than that of silk, of great strength, and more perfect if possible than glass or any of the metals in its elasticity. Such a thread, if found, would be valuable not only for my experiments, but for almost all investigations, the accuracy of which is limited by the defects of our present apparatus.

My first endeavours were to make glass fibres finer than spun glass, and I concluded from my experiments that the conditions for success were the employment of a very small quantity of melted glass, a very high temperature, and the instantaneous production of a very high velocity. These conclusions I tested by the very simple process of attaching a fine rod of glass to the tail of an arrow, and then, when a bead had been melted with the oxy-hydrogen jet as large about as a pin's head, shooting the arrow over the longest range that I could conveniently make use of. Under these conditions the bead of glass remains suspended in mid-air under its own inertia, while the arrow goes on its journey; but when the arrow has struck the target, there is found floating in the air, far too fine to fall quickly, a delicate thread of glass, no thicker sometimes than a single spider line. If, instead of glass, a fine rod of melted quartz is fastened to the arrow, held at the free end with the fingers, and heated to the highest temperature attainable with the oxy-hydrogen jet, then a thread of quartz can be produced in the same way, and, as you see, the process is perfectly simple and easy. [A quartz fibre was then shot, and its existence made evident by fastening to it a gummed label.]

Now having obtained quartz threads, it is necessary to describe their properties. In the first place they can be made as fine as anybody is likely to want them; threads as fine as one ten thousandth of an inch are perfectly manageable, and can be had in pieces yards long, if required. Threads as fine as one hundred thousandth of an inch can be made, but are not easy to handle. This will be the more readily understood when I say that a bundle of 10,000 of such threads would be no larger than a single piece of spun glass. How fine the finest ends of some of the fibres that I have made may be, there is no means of telling. Dr. Royston Pigott believes them to be less than a millionth of an inch in diameter, and I have concluded from their appearance in the microscope—but this is hardly the right expression, for I have not been able to trace them to their end because they are far too fine—that they cannot be much more. These certainly

* "Phil. Mag.," Dec., 1886.
† "Phil. Mag.," Jan., 1887.
‡ "Phil. Mag.," June, 1887.

cannot be handled or used as torsion threads. As it is impossible to realise what numbers such as a million really mean, I may say that a piece of quartz one inch long, and one inch in diameter, would, if drawn into thread one millionth of an inch in diameter, produce more than sufficient to go all round the world 650 times, or an express train travelling without ceasing would take thirty years before it could unwind this amount if wound on a reel.

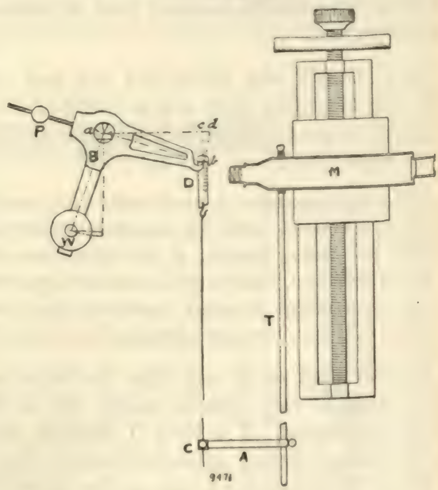
The next quality that I must refer to is the strength of quartz threads. Experiments made with great care by Mr. Highfield and myself in the physical laboratory show that, as in the case of glass, these threads became stronger in proportion to their size as they are finer. A quartz fibre rather finer than spun glass, with a diameter of 6·9–10,000ths of an inch, has a strengh of 8×10^9 dynes to the square centimetre, or of 51·7 tons to the square inch. A fibre with a diameter of 19-100,000ths of an inch has a strength of $11{\cdot}5 \times 10^9$ dynes to the centimetre, or 74·5 tons to the inch. This is actually stronger than ordinary bar steel. Thus quartz fibres have the two valuable properties that they can be made as fine as you please and are of enormous strength. We have also found that when the outside of fibres of spun glass or quartz is dissolved with hydrofluoric acid, the strength goes on proportionately increasing as they get finer, showing that in the case of fine fibres the great tenacity is not due to a vitreous skin. Fibres, originally of various sizes, after being dissolved away till they break with a certain weight, have approximately the same strength, which shows that the great strength of fine fibres is not due to their being formed under pressure. (The pressure due to a surface tension equal to that of water would in a fibre one millionth of an inch in diameter be equal to 60 atmospheres, and in thicker fibres less in the same proportion.) These experiments point to the conclusion not verified in other ways, that there is something analogous to surface tension in the solid fibre.

The next point to consider is the elasticity of the fibres both to stretching and to torsion. Experiments in this direction, I hoped, might throw some light on the cause of the enormous strength of fine fibres. I have on the table the apparatus that I devised for this purpose (Fig. 8).*

* The block of this figure has been kindly lent by the Editor of *Engineering*.

The apparatus made by Hilger consists of a microscope cathetometer shown in the figure at M, which can be made to traverse a vertical slide by means of a fine screw having a micrometer head, the divisions of which are capable of being read directly to the one-thousandth of a millimetre. To the end of the microscope farthest from the eye-piece is attached the vertical tube, T, which carries at its lower end an adjustable arm, A, fitted with a clamp, C. To the end of a separate bracket, fastened to the stand of the cathetometer, is fixed the block, *a*, which supports, by means of a knife-edge, the beam, B, which is weighted with a gravity-bob, W, and carries on a second knife-edge, *b*, the micrometer scale, D, the opposite end of the lever being counterpoised by the adjustable weight, P. The fibre to be tested has attached to it a pin at each end to facilitate its being fixed in the apparatus, it being stretched vertically between the scale, D, and the clamp, C.

FIG. 8.



When the micrometer head is turned, the cathetometer, M, is lowered, carrying with it the tube, T, and thereby putting a tensile strain on the fibre, which draws down the lever, B, being itself stretched under the increasing pull of the lever. The extension of the fibre is measured by the movement of the scale, D, across the field of the microscope, and the deflection of the lever, B, is a measure of the force that is being applied to the fibre. This is obtained by subtracting the amount of extension of the filament from the distance traversed by the microscope, which latter may be determined with the greatest accuracy by the readings of the micrometer head.

In adjusting the instrument the slide is first

made vertical by levelling screws, the accuracy of the levelling being determined by means of a spirit level placed in different azimuths on the top of the micrometer head. The counterweight, P, is next adjusted until the knife edges at a and at b are both in the same horizontal plane, and this adjustment is made when the scale D and the upper attachment pin of the fibre are in their proper position, and the microscope is focussed so as to give a sharp definition of the divisions on the scale. The fibre having been attached to the upper supporting pin and suspended in its place, the length of the arm, A, is so adjusted that the lower supporting pin of the fibre hangs freely in the axis of the clamp, C, which is then tightened, and thus perfect verticality at the commencement of the pull is ensured. The micrometer head is then slowly turned, readings being taken as each division of the scale D traverses and coincides with the cross wire of the microscope, and the force which thus extends the fibre by each increment of one-twentieth of a millimetre is determined in the following way.

If the adjustments of the instrument have been made in the manner described above, the moment due to a vertical pull is proportional to the cosine of the angular displacement of the beam, while that due to the gravity bob and the other portions of the beam varies as the sine of that angle, the actual tensile force applied at D being proportional to the tangent of the inclination of the beam. The vertical distance $c\,b$ is a measure of the sine of the inclination, and when the angular displacement is small this distance is practically the same as the tangent of the angle, and it may be corrected to measure the tangent if very great accuracy be required. The true value of the force corresponding to various values of $c\,b$ may, however, be more easily found by attaching weights to D, and observing, by means of the microscope and scale, the weights which produce corresponding deflections.

In this instrument there are two apparent sources of error, which, however, do not in any way affect the accuracy of the measurement. In the first place, it is evident that as the beam is deflected, the point b becomes more and more distant from the microscope, and the pull on the fibre ceases to be vertical, but it must be also noticed that in doing so the scale D is carried out of the focus of the microscope, which has in consequence to be adjusted by being moved forward to the exact amount which the scale had receded by the movement of the beam, and thus the arm, A, carried by the end of the microscope is moved forward to an equal extent, the scale comes again into focus, and the fibre becomes again vertical.

Again, in the case of the tube, T, being very long, it might happen that the spring of the tube and of the arm, A, might cause the fibre to appear more stretched than it really is, but the error due to this cause can be perfectly eliminated by finding, in the course of the experiment, the force that is being applied to the fibre, and afterwards placing weights on C until a pull of the same amount is obtained. As a matter of fact, however, with ordinary fibres, the further movement of D under these circumstances is not observable.

Messrs. Gibson and Gregory used this apparatus for a very large number of experiments, which they conducted with great care and exemplary patience. They examined fibres both of glass and quartz. The mean value of Young's modulus found for spun glass is $5{\cdot}16 \times 10^{11}$, and for quartz about 6×10^{11}. The modulus of torsion was observed in the usual way, with the following results:— Spun glass, $3{\cdot}22 \times 10^{11}$; quartz, $3{\cdot}74 \times 10^{11}$. The rigidity (obtained by dividing these figures by $\left.\frac{\pi}{2}\right)$ is for spun glass, $2{\cdot}05 \times 10^{11}$; for quartz, $2{\cdot}38 \times 10^{11}$.

There seems a good deal of variation between different specimens, but these figures go to show that the elasticity of spun glass is much the same as that of glass in mass, and that quartz is very slightly stiffer than glass.

The only serious fault of glass fibres is the fatigue, which I experimentally showed in the early part of this lecture. I shall now perform the same experiment with quartz, and it will at once be evident that the fatigue of quartz, if it exists, is nothing in comparison with that of glass. As a matter of fact, this fatigue does not cause a change of zero of 1-50 of the corresponding change in the case of glass, and it may be less; but practically it is quite beyond our power of observation to detect any effect with such deflections as are met with in the practical use of instruments.

Quartz, then, has these properties. It is, when in fine threads, stronger than the metals, and so a finer thread of this material will carry a given weight than can be made of any other material. The torsion of quartz is but little more than that of glass and is less than a

third of the torsion of steel, therefore the small torsion due to a very fine fibre is made smaller still on this account. Then since it is practically free from fatigue, since it is not affected by moisture in the air, nor liable to all the vagaries for which silk is so famous, and since it may be easily made in pieces of almost any length, there is no reason why silk should still be used. I have now for some time been making experiments with quartz fibres,* that would have been, as far as I know, quite impossible with silk, without meeting with any trouble, and to me it seems, in the light of my experiments, to be unwise to use silk in instruments of precision. I may add that I am not trying to find a market for anything of mine, for these fibres are not patented—they are free to the world to use.

As an inventor or discoverer is apt to form too favourable an opinion of anything that he may have done, I shall refer to the experience of others with quartz threads.

Professor Threlfall has found it possible to improve the galvanometer by various modifications until it is from 100 to 1,000 times as accurate and delicate as a measuring instrument as any that can be made in the usual way. It was only when he used quartz that he was able to make any real advance; with silk instead of quartz his instrument was practically no better than any other. This experience I particularly value because he was at first very sceptical as to the truth of what I had said about the value of fibres of quartz.

Professor J. Viriamu Jones writes:—"You may indeed say that I find the quartz fibres an advantage after silk. I could not get in my low-resistance galvanometer (with an ordinary Elliott needle) a constant zero with a period of oscillation of three or four seconds; with your fibre I can get constancy with a period of at least twenty seconds. It has always seemed to me that in a very weak field a galvanometer needle suspended by silk indicates that the silk twists and untwists in the most unreliable and variable manner. Your fibre, on the contrary, in the weakest field I have put the needle in, maintains constancy of equilibrium. ....You are a benefactor of mankind."

These are not the only instances in which my experience has been fully confirmed by others. It is in consequence of these independent opinions that I have ventured to express myself in the way that I have with regard to silk.

There is another property of quartz fibres and rods which may be found to be of some importance. A stick of clean quartz that has been melted and drawn out is in a damp atmosphere almost a perfect insulator, while a piece of glass is, judged by the behaviour of the leaves of gold, an almost perfect conductor. Not only may quartz be used in a damp air, it may even be dipped in water, and gold leaves may be hung on it immediately, when it will be found to insulate just as well, even though the water is still visibly clinging to it.*

This high insulation of quartz, even in a wet place, may make it possible to construct electrostatic measuring apparatus which will always be ready for work, and in which sulphuric acid need not be used. But here I am leaving my subject. Going back to quartz threads, it is a curious fact that the ends of many of these become highly electrified in the process of manufacture. Whether it is the friction in passing through the air, or whether it is electrification produced in the oxy-hydrogen flame, I do not know, but the fact remains that the floating ends of some of these threads are very highly electrified, and the electricity, small in amount on account of their extremely small capacity, is absolutely unable to escape, but it makes its existence evident in a very curious way. The ends of these fibres, as seen floating, often assume a spiral or wavy form, and these screws serve as excellent instruments for revealing the presence of electrification, which I believe is too small in quantity to show on any apparatus. If you gradually approach the hinder end of one of these electrified screws it suddenly shoots itself out, and the fibre is projected on to the approaching body. This may be repeated several times.

It is for this reason, I believe, that it is possible to gather the extraordinary material which you now see upon the screen. If you take a strong oxygen hydrogen jet, and keep joining and separating two sticks of quartz in the flame, a very great length of a complex cable is shot away, but it is so fine that it will float away and be lost. If, however, you place in the line of the flame, but a foot or two away, a retort stand or other convenient support, you will find this covered with long threads of this multiple cable, consisting of fibres of all sizes, from one five-thousandth of an inch downwards. By varying the size of the flame, the proportion of the gases, and the place at which the quartz is held, you can produce anything between this complex cable, down to those fine

* "Proc. Roy. Soc." Vol. 46, p. 253.

* "Phil. Mag." Aug., 1889.

tapering tails which no microscope can follow to the end.

The following hints may be of use to those who are going to mount apparatus upon fibres for the first time. Having chosen a fibre of the right diameter, and longer than is ultimately required, the first thing to do is to fasten a small fragment of gummed stamp paper to one end. This acts as a weight, and makes the following processes more easy. The upper, or fixed support must next be fastened to the free end of the fibre. I prefer a common blanket pin passing through a cork to any of the more elaborate contrivances in common use; however, whatever is going to be used for the fixed support should be pointed at the lower end.

If the needle or other thing to be supported is very light, *i.e.*, nowhere approaching the breaking weight of the fibre, shellac varnish is the best thing to use as the cement. Just moisten the last five millimetres above the pin with this varnish, and—holding the fibre near its free end in one hand, and the pin in the other, with the little finger of one hand resting against the little finger of the other, for the sake of steadiness — immediately apply the fibre to the varnished surface, to which it will stick. Then pull it endways through a distance of half a millimetre about, to make sure that when all is dry there will be no sudden bend in the fibre. A hot piece of wire, or knife, or pair of pliers must then be applied to the pin above the varnish, so that the heat may be conducted down slowly to drive off the remaining alcohol and melt the shellac. After this the fibre is securely held at that end.

If the thing to be supported is very heavy, varnish is not so good as melted shellac, but this is much more difficult to apply. The pin must be warmed and smeared near the joint, and while hot the fibre must be applied and slightly pulled. Whether varnish or melted shellac is used, it is essential to work in a proper light. A table in front of a large window in the day, or supporting one or two movable gas burners at night, will do perfectly provided that a really black background exists, upon which the fibre may be made evident. Black velvet, or paint, or paper is no use whatever. The only background that does well, and one that is easily arranged, is the darkness inside a drawer just pulled out an inch or two.

Assuming that one end of the fibre is properly fastened, the next thing is to determine the exact length required. For this purpose I always make a drawing on a perfectly clean and smooth board, showing the point of the pin at one end, the extreme end of the thing to be supported, and the position of the mirror or whatever determines the length of the fibre.

The holding-pin is then raised up until the paper weight is hanging in the air. This is then allowed to rest on the board, and slowly dragged along until the point of the pin is exactly over the corresponding mark on the board, and the paper wafer is in the line of, but beyond, the other mark. The fibre is then straight, and must pass over the mark which indicates the upper end of the apparatus that is to be suspended, though, of course, nothing can be seen. A knife is then drawn across the board, say five millimetres beyond this mark. By this means the fibre is cut, and the five millimetres are left for the purpose of attachment. The needle or suspended thing is then fastened in the same way as the pin. It is well then to drag the needle along until the mirror is exactly over its place on the drawing, and see if the upper pin is also in its place. By warming either pin, and pulling the fibre, a slight alteration can be made if necessary.

The paper weight with the remainder of the fibre may then be taken up, and the free end fastened on a microscope slide and labelled, so that the diameter of the fibre may at any time be found. If the fibre, illuminated by a distant light, had been examined with a prism first, and the dark bands of the spectrum* had been found straight and uniform from one end to the other, and it is only such fibres that I use, then the diameter must be the same over the whole length.

Though I have given a large proportion of the time at my disposal to this one part of the subject, I hope that it will not be considered that I have occupied more time than is demanded by the importance of the subject or the value of the results, of which some are now published for the first time.

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* "Proc. Roy. Inst.," June 14, 1898.

LECTURE III.—DELIVERED APRIL 8, 1889.

Of the instruments for detecting feeble effects of temperature, the best known are those depending on the electromotive force set up in a metallic circuit composed of different metals when there is a difference of temperature between the junctions of the metals.

The elementary metals may be arranged in a series, with bismuth at the beginning and antimony at the end, so that if a circuit is made of any two, an effective electromotive force will be found tending to send a current across the warmer junction from the lower to the higher metal in the series. The greater the difference of temperature, and the further the metals are apart in the series, the greater, in general, will be the effective electromotive force. When the temperature differences become large, it is found that the electromotive force is not exactly proportional to the difference, and in some cases not at all proportional, so much so, that if the hot junction is heated, or the cool junction cooled, beyond a certain point, instead of an increase there is actually found a decrease of electromotive force. Though these departures from proportionality can be completely represented by the thermo-electric diagram of Prof. Tait, there is no occasion to consider this at the present time, for with such very small differences of temperature as we have to deal with in thermopiles and similar apparatus, the electromotive force is, as far as experiment can show, exactly proportional to the temperature difference for any mean temperature, and the effect of such small changes of temperature as are met with in a laboratory from day to day, while very small, can, if necessary, be exactly allowed for. We may, therefore, with propriety use the old-fashioned expression, and say that the "thermo-electric power" of any pair of metals is so much, and that the further the metals are apart in the series, the greater is their thermo-electric power. Turning, then, to the tables given in the text-books, bismuth and antimony being at the two ends, have a greater thermo-electric power than any other pair of simple metals. Iron and copper, which are intermediate metals, form, at ordinary temperatures, a combination of which the thermo-electric power is about one-seventh of that of bismuth and antimony.

It is impossible at present to give any reason why the several metals should have the thermo-electric properties that they are found to possess. In the case of some of the alloys the properties are the reverse of what might be expected. Thus, though bismuth is the most positive, and antimony the most negative of the metals, an alloy composed of thirty-two parts bismuth and one part antimony is more positive than bismuth itself; though antimony is the most negative of the metals, and bismuth the most positive, and tin is nearly neutral, an alloy of twelve parts bismuth and one of tin is more negative than antimony. The first of these alloys, and one almost the same as the second, are stated by Lord Rosse* to be the alloys which, at that time, Messrs. Elliot Bros. used in their thermopiles.

I have, with the help of Mr. Burbidge, examined these two alloys, and find that at ordinary temperatures they have a thermo-electric power one-third greater than that given by bismuth and antimony. They have this further advantage, especially over antimony, that they are both easily fusible, and can be cast in thin leaves between smoked glass plates.

The electromotive force acting in a circuit composed of these alloys, when the temperature differences between the junctions is one degree Centigrade, is about one sixty-thousandth of a volt. As this is an enormous electromotive force for a delicate galvanometer, an exceedingly small temperature difference can be detected.

To still further increase the electromotive force it is usual to make thermopiles with a very large number of pairs of bars having their alternate junctions near together. Heat thus falling upon one side of the pile will warm the alternate junctions, so that the electromotive force due

* "Proc. Roy. Soc.," xviii., p. 553.

to all the pairs will be added together. In the pile now on the table, made by Elliot, there are eighty pairs of bars, and these are packed into a space only two centimetres square. Each bar has a section of about 2×1 millimetre.

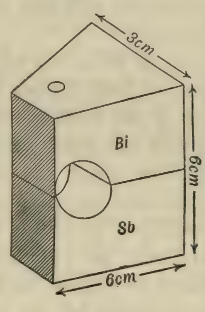
As an example of the sensibility of the thermopile, I may give the following results obtained with the ordinary standard apparatus, namely, the thermopile referred to above, and an Elliot reflecting galvanometer of low resistance made to be used with the pile. When the magnet was so adjusted that the needle had a period of oscillation of six seconds, the heat from a candle flame at a distance of five feet produced a deflection of 110 millimetres of the spot of light, on a scale one metre from the mirror, that is with the reflecting cone in position. Without the cone the deflection was 31 millimetres. The candle flame is perhaps not a very scientific or exact source of radiant heat, but it is very convenient, and serves well when the object is to compare one instrument with another. If an improvement in an instrument is of so slight a character that the small variations of heat radiated from the flame of a candle of any definite make are liable to mask its effect, then the improvement cannot be of much consequence to anybody; but if by any means an instrument can be made ten or a hundred times more delicate, then the greatest variations in the candle flame will be insufficient to materially affect this proportion, and so, for practically testing the value of an instrument, there is no occasion to set up a more constant source of radiation.

I have given the results obtained with the standard apparatus so as the more readily to trace the value of more modern development. The resistance of all the bars of the pile and of the wire of the galvanometer is one of the factors that determines the strength of the current. If by any means the resistance could be reduced, the current would be increased in the same proportion, that is if the temperature difference remained the same.

Professor Forbes has devised a simple instrument,* which he calls a thermo-galvanometer in which the resistance is reduced enormously. He reasoned as follows. Imagine one pile of twenty pairs, and a second having the same outside dimensions, but consisting of one pair; then for any temperature difference, if the electromotive force and resistance of the pair are each called unity, the electromotive force of the pile would be twenty, and its resistance four hundred. Now, since the resistance of the galvanometer most suitable for any pile is equal to that of the pile, the total resistance of the pile and galvanometer should be eight hundred. If now the two ends of the pair are united, and not connected to a galvanometer at all, the currents in the two cases should be, by Ohm's law, $\frac{20}{800} = \frac{1}{40}$ in the case of the pile and galvanometer, and $\frac{1}{1} = 1$ in the case of the pair; so that with a simple pair the current should be forty times as strong as with a pile of twenty pairs and a galvanometer. On the other hand, since there are many turns of wire in the galvanometer, the current may go round the needle so many times as to more than compensate for its feebleness; and though the simple pair so formed as to enclose a needle may not be more sensitive than the usual pile and galvanometer, yet on account of its great simplicity and cheapness, it may be of value in some cases.

The form which Professor Forbes gave to his pair is shown in Fig. 9, from which it

FIG. 9.



will be seen that the sectional area of the metal is very great in all parts except at the face which is to be exposed to radiation. This face is filed away until the linear junction so formed is so thin that with further filing there would be risk of destruction. The face is then blackened so as to make it absorb heat freely. A hole drilled through the upper block allows the needle and mirror, which hang within the cylindrical hole in the block, to be supported by a thread from above.

Messrs. Nalder Bros. have kindly lent me one of these instruments of their make, which Mr. Burbidge and I have examined. Whether

* "Proc. Roy. Soc.," Feb. 18, 1886.

this is a good sample of the instrument I do not know, but in sensibility the instrument is far behind the thermopile and galvanometer. That it does not come up in sensibility to the value that the consideration of Ohm's law alone would lead one to suppose is due to the following causes. In the first place, whatever is done to reduce the electrical resistance of the bars applies also to their thermal resistance, and so, since this also is reduced to the greatest extent possible, the heat which falls on the hot junction can escape freely, and so the hot junction is not made so hot by the absorption of heat at a given rate as it would be if the thermal resistance were greater. Again, when an electrical current passes in a thermo-electric circuit, heat is carried from one junction to the other by the current, and the quantity of heat which is so carried is proportional to the current strength. Since the current density in Professor Forbes's instrument is at least twice as great as in the usual arrangement, heat is electrically carried from the hot to the cold junction at at least twice the rate for a given temperature difference; and so, if this were the only cause of equalisation of temperature, the hot junction would be heated to less than half the extent above the cold that it would be if the current density were at the usual rate. However, I do not think this electrical transfer of heat is important in comparison with that due to ordinary thermal conductivity.

The only point in which my experience does not confirm that of Prof. Forbes is in the dead-beat character of the instrument. He states that on account of the small resistance of the block of metal in which the needle is suspended, the electrical currents induced by its motion are sufficient to make it dead beat. The instrument that we examined showed by no means a large decrement, in fact, the great number of oscillations of the needle were a serious inconvenience.

Now returning to the ordinary pattern of thermopile and galvanometer, the question is worth asking whether it is capable of improvement. Are the number of bars that the instrument maker happens to have employed the best number? Are the number of windings and the thickness of the wire in the galvanometer the best for the purpose? At first let the space filled with the thermo-electric metal in the pile, and the space round the needles in the galvanometer filled with wire be supposed constant, then there are two problems each of which has the same answer. They are, given a thermopile find the best galvanometer, and given a galvanometer, find the best thermopile. Let nothing be altered except the number of pairs in the pile, or of turns in the galvanometer, then all other kinds of variation due to conduction of heat, radiation, &c., or to the magnetism or moments of the needle for the time do not exist.

If the pile is made with twice as many bars, its electromotive force will be doubled, but its resistance will be increased in a fourfold ratio. The increase of resistance is objectionable, while the increase of electromotive force is the reverse. There must be some number of bars which will be most advantageous. This is the case when the resistance of the pile is equal to the resistance of the galvanometer and connecting wires. The solution of the converse problem is the same, the number of turns and size of wire should be so chosen that the resistance of the galvanometer is equal to the resistance of the thermopile and connecting wires. These results are not quite true, because of the electrical transfer of heat, known as the Peltier effect already referred to. On this account the galvanometer resistance should be slightly higher than the resistance of the thermopile, but the heat transfer due to this cause is, as already mentioned, small in comparison with that due to thermal conduction.

The solution found at present is only relative. Given a galvanometer the best pile may be found, or given a pile the best galvanometer may be found, but we do not yet know whether both should have many bars and turns of wire, or few bars and turns. Judging by the practice of instrument-makers, who crowd eighty pairs of bars into so small a space, a great number would appear to be best.

Now, imagine a thermopile and galvanometer exactly suited to one another, and a second pair of the same pattern in every respect, except that they have just half as many bars and half as many turns of wire, each filling, of course, the same space. Then the total resistance in the second case will be one quarter the resistance in the first, while the electromotive force will be one half; the current therefore will be twice as strong, but as it goes round the needle only half as many times it will produce exactly the same effect.

This is a really true comparison, for the Peltier effect, heat conduction, radiation, &c., are the same in both cases, it therefore follows that since half the number of bars and turns is equally good, the same will be true of half the

number again and so on, until there is either only one pair of bars in the pile, or only one turn of wire in the galvanometer. It would, therefore, appear that all the trouble and expense of making so many pairs of bars in a thermopile is a waste, and that one pair is just as good as a million. This result is only true so long as the resistance of the connecting wires is sufficiently small to be neglected. If, as is sometimes necessary, the thermopile is some distance from the galvanometer, and if also flexible wires are essential, then, owing to the resistance of the connecting wires, many bars should be used, for just as they are increased in number does the resistance of the connecting wire interfere less and less. There is another reason of greater practical importance why many bars are advantageous, and that is that as the working electromotive force of the pile is increased, so, in proportion, do accidental electromotive forces produce less disturbing effects. For instance, at every binding screw there is a thermo-electric junction, at which accidental temperature changes, due to hot or cold draughts, handling, &c., set up electromotive forces, which may be exceedingly troublesome. Or, again, if the connecting wires are any of them apt to swing about, each one in cutting through the earth's magnetic field will cause an electromotive force to be set up which may largely disturb the galvanometer needle.

It is only in the cases where these disturbing causes are met with that any advantage is gained by the use of a large number of bars. If they can by any means be avoided, then a single pair of bars does just as well as a large number.

The next thing to consider is whether the single pair, which is as good as a pile of eighty pairs if the galvanometer and connections are properly arranged, and which is at present supposed to occupy the space of eighty pairs of bars of the usual size, can be made more effective by increasing or decreasing its sectional area, its length remaining the same. Let it be reduced until it is half the thickness and half the breadth, then its resistance will be four times as great as it was, and the galvanometer, altered so as to have four times the resistance also, will have twice the number of turns. If it is possible to concentrate all the heat which would have fallen on the larger surface, also upon the smaller, then, since practically all the heat which falls on the hot junction is carried away by conduction, by radiation from the face, and by the Peltier action of the current, which each now go on at one quarter the rate for any given difference of temperature, the hot end will acquire four times the excess of temperature; the current, therefore, will be the same, but it goes round the needle of the galvanometer twice as often, and so the deflection will be doubled. In the same way, if the bars are made ten times as small each way, the deflection will be ten times as great.

If, on the other hand, the bars received the proportion of heat due to their smaller surface, that is one quarter and one hundredth in the two cases, then the deflections would have been one half and one tenth instead of two and ten.

Lord Rosse* has obtained the effect of concentrating on the ends of small bars the heat which corresponds in quantity to the area of large bars by simply soldering the ends of a pair of very thin bars to the centre of a thin disc of copper, the opposite face of which is blackened to receive the radiation. He found that with thin copper discs half-an-inch in diameter there is hardly any loss of effect, owing to the want of perfect heat conductivity in the copper. This plan has the further advantage that the soldered surface or junction is on the heated surface, and not, as usual, buried to a depth of a millimetre or so in the pile. In the latter case a superficial warming gives rise to currents which partly return at the back of the warm junction so that the full current to the galvanometer is not produced until the pile is heated to the bottom of the junctions. Lord Rosse fully realised the advantage of small bars, a single pair of which he has used in his investigations on the heat of the moon. He has experimentally verified the conclusions which he found theoretically to be necessary, namely, that as the bars are made thinner, the junction not only produces a greater effect upon a suitably arranged galvanometer, but, in addition, the final temperature, and, therefore, the steady current is much more rapidly developed.

For some purposes the smallest heat receiving surface is sufficient, as in the experiments of Dr. Huggins and Dr. Stone upon the heat of the stars. In this case the image of the star produced in the telescope is so small that all the heat can be brought to a surface no larger than the point of a pin, so here especially a single pair, and that as fine as possible is better than any pile.

By this reduction in the size of the bars one

* "Proc. Roy. Soc.," xviii, p. 553.

of the disturbing elements in the argument that has been used is made of less importance, I mean the effect of the resistance of the connecting wires. If the bars could be made so fine that their resistance would be equal to that of an ordinary pile, then the resistance of the connecting wires would, for the purpose of comparison, be eliminated.

Wishing myself to put this argument to the test of experiment, I devised and made a special form of junction on Lord Rosse's plan, but with bars made of the alloys already described far finer than have been used before. They have a section of about one-twelfth of a millimetre each. They are soldered at one end to a piece of copper foil about one-third of a square centimetre in area, and at the other end to two large pieces of copper foil, separated by mica and pressed together. This is to ensure that the unexposed ends of the bars shall be at the same temperature, and that any uncertainty due to heat brought to one bar more than the other by the connecting wires shall be eliminated. As the bars are so fine that they could not carry the copper foil without risk, amounting almost to certainty, of being broken, I have had to support the copper foil independently by slinging it on stretched fibres of spun glass. These hold it securely, but do not carry heat away from it to any appreciable extent. Connecting this junction with the galvanometer that was made for use with the pile of eighty pairs, a galvanometer which is not at all suitable, we found that a candle flame five feet away produced a deflection of eleven millimetres, or one-third of the deflection produced by the pile. The exposed surface was, however, only one-twelfth of that of the pile. It appears, then, that the pair with an unsuitable galvanometer did four times as well as the pile with its own galvanometer.

Mr. C. C. Hutchins has given a short account* of an instrument which is simply a junction of steel and copper ribbon 1 × ·03 mm. at the focus of a concave silver-on-glass mirror eight millimetres in diameter. He states, as a record of its performance, that the hand held six inches away produces a deflection of 30 divisions, and a lighted match at six feet drives the needle to the stops. I do not think Mr. Hutchins has done justice to the idea, for with such metal as steel and copper he might have made them far thinner, or with a thickness but little greater he might have used the alloys of bismuth, which have a thermo-electric power seven times as great.

It is necessary to use a galvanometer with a thermopile, and the delicacy of the former is just as important as the proper construction of the latter. In the first place, assuming that the ordinary form of galvanometer is to be used, the question arises—With what size of wire should the coils be wound? What is required is that the galvanometer should be so wound as to have some predetermined resistance, and yet have as many turns of wire as possible, especially near the inside, where each turn produces a far greater effect than one of the outer turns. Clerk Maxwell has shown that so long as the insulating layer occupies a small proportion of the space filled by the wire, which is nearly true in low resistance galvanometers, or that it has a thickness proportional to the thickness of the wire, the best effect is obtained when the diameter of the wire is made to vary in direct proportion to its distance from the axis. A coil so wound is said to be graded. Of course, in practice, a new size of wire is not taken for every layer, but a few sizes are employed in their proper places.

The next question that may be asked is—Why should the ordinary size of coil be selected? Why should its interior or exterior limits be just those commonly used? Imagine two coils geometrically similar in all respects, but one of four times the dimensions of the other, then the number of turns in each will be the same, the resistance of the small one will be four times as great as the resistance of the large one, and the magnetic field at the centre of the small one will for a given current be four times as great as the field in the large one. Now, to make the coils strictly comparable, they must each be made of the same resistance. If the number of turns in the small one is halved, the resistance will then become the same as the resistance of the large one, and so the same current will be sent through it as through the large by any pile with which it may be connected. The magnetic field at the centre, however, will now be twice as strong in the small as in the large coil, and therefore the deflection will be twice as great. If the space occupied by both coils, so formed as just to fit within one another, were filled with wire, graded, and of such a size as to have the same resistance still, then the magnetic field at the centre would be $\sqrt{5}$ or 2·24 instead of 2, that of the large one alone being considered unity; so the

* "Phil. Mag.," Jan., 1888.

relative deflections produced in the three cases and the weights of the coils would be:—

	Large coil.	Small coil.	Both.
Deflection	1	2	2·22
Weight..........	256	1	257

The figures given for the double coil hardly represent the best that could be obtained with such an extended coil, because the shape is not of the best; but, while a slight advantage could be gained by employing also the best shape, the figures are sufficient to show what a great advantage is obtained by reducing the interior of the coil, and how little is gained by enlarging it outwardly.

Since the needle or needles of the galvanometer must be suspended within the coils, it is evident that it is not possible to reduce the aperture in the coils without at the same time reducing the needle also. The question then arises whether shorter needles have any advantage. If the needles are reduced to half their length their moment of inertia will be reduced to one-eighth of the previous amount, while the magnetic moment will be reduced to one-half, or perhaps to rather more than half. If the controlling force which brings the needle to rest, whether due to a magnetic field or to torsion, is so modified as to give any definite period, then it will have to be proportional to the moment of inertia, and will therefore, in the case of the short needle, be one-eighth of what it would have to be in the needle of double length; the consequence is that under these conditions—that is, of definite period—a given current in the coils will produce a deflection four times as great in the smaller needle. If both coil and needles are reduced in the same proportion, that is to $\frac{1}{n}$ of the original size, then resistance and period remaining the same, the deflection due to a given currrent will be $\sqrt{n^5}$. The only reason why this general reduction of dimensions has not been made before is that as the needles are made smaller or shorter, the irregularities of the silk suspension become of greater importance, so much so, that with needles only half the length, and with the same period, the disturbance would be eight times as great, and so for other dimensions in the proportion of the inverse cube. As it is, with the ordinary size of needle used in the galvanometer, the effect of the silk is sufficiently obvious with slow periods, and so no good would result from further reduction, but rather harm. On the other hand, with a quartz fibre, the full advantage of the smallest galvanometer that can be made is readily obtained.

There is another question in connection with the galvanometer, namely, whether it is best to use an astatic combination or a single needle. If the astatic needles are each placed as usual within coils, then in a low resistance galvanometer the deflection would be only $\sqrt{\frac{1}{2}}$ or about ·7 times as great as it would be if the same current were passed round a single coil of the same size, and of the same resistance as the two with only one needle, provided that the period of oscillation remained unchanged; or with a second needle outside the coil, the deflection would be rather more than half of this latter result. I have here supposed that the moment of inertia of the mirror is not more than a small fraction of that of the needle.

To put these conclusions to the test of experience, I have made a very small galvanometer, with a coil about the size of a threepenny bit, with a triple needle within it. The needle was rather less than three-sixteenths of an inch long, and weighed, with the mirror and connecting stem, less than three-quarters of a grain. This was suspended by a fine quartz fibre three inches long, and I had no difficulty, partly by screening and partly by a counteracting magnet lying on the table, in almost immediately producing a period of as much as ten seconds. This little galvanometer was connected with the thermo element already referred to, which was exposed to a candle at five feet. The deflection obtained when the period of oscillation was about seven seconds was 67 mm., while with a period of ten seconds it was 126 mm. Since the heat received is exactly one-twelfth of that which would fall on the pile placed at the same distance, and which produced a deflection of 31 mm. on the standard pattern galvanometer, it is evident that with the same amount of heat the standard instrument would have produced a deflection of $2\frac{1}{2}$ mm. only, and so it appears that the combination of simple element and very small galvanometer is from 26 to 50 times as sensitive as the much more elaborate and expensive apparatus which is always used.

Though a single needle or group of needles is certainly preferable to an astatic combination, where sensibility only is the object, or where disturbing magnetic changes do not occur, there is a great practical advantage in using an astatic system in most cases, because

if there is a slight magnetic disturbance, whether due to natural causes or to the movement of iron in the neighbourhood, the effect is by no means so marked when an astatic, or nearly astatic, combination is used, as when a single needle is suspended in a field where the earth's magnetism has been almost entirely neutralised by an adjustable magnet.

There is one other consideration on which I have at present only very lightly touched, and this is the time that elapses after the heat falls upon the instrument before a fairly steady reading can be obtained. This is generally determined by the mass of the junction or junctions which have to be heated. These go on rising in temperature until the loss of heat is equal to the gain. The greater the mass the longer in general is the time that must elapse before a steady temperature is obtained. In some cases, especially when the galvanometer needle has a long period and a slight logarithmic decrement, and when the mass of the thermo-junction is but small, the time of coming to rest may depend chiefly on the galvanometer.

Mr. Burbidge and I have determined this time in the case of the various instruments referred to in this lecture. The figures are of necessity mere approximations. They are:—

	Seconds.
Thermo-galvanometer	200
Elliot's pile and galvanometer	120
The small thermo-element and Elliot's galvanometer	30
The small thermo-element and the small galvanometer	60

These figures are important, not so much because if an instrument requires twice as long before a reading can be obtained than another it will also require twice the quantity of heat on this account, but because time is a very important factor when heat measures have to be made under the usual variable conditions. If a measure can be made in half the time, the uncertainty of the position of rest due to changes of temperature which cannot be prevented, is certainly on the whole not more than half what it would be in the double time—it may be even less—and so the accuracy or degree of trustworthiness is greater in proportion as the time which it is necessary to wait is less.

Mr. Burbidge has prepared the following Table, which shows in round numbers at a glance the value of the several combinations described in this lecture:—

	Actual Deflection.	$\frac{\text{Deflection.}}{\text{Heat received.}}$	$\frac{\text{Deflection.}}{\text{Heat} \times \text{time.}}$
Thermo-galvanometer (with cone)	$\frac{1}{2}$	$\frac{1}{10}$	$\frac{1}{15}$
Do. do. (without cone)	$\frac{1}{24}$	$\frac{1}{5}$	$\frac{1}{4}$
Elliot pile and galvanometer (with cone)	$3\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$
Do. do. (without cone)	1	1	1
Pair with Elliot galvanometer (without cone)	$\frac{1}{3}$	4	$17\frac{1}{2}$
Do. small do. (without cone)	2	24	48

The falling off of the figures in the second and third columns, where a reflecting cone has been used, shows that it fails to usefully reflect all the heat which falls upon it. It ought never, in my opinion, to be used when definite results are required. I do not lay any stress upon the exact value of these figures, especially those in the third column; but, taking the ordinary apparatus without a cone as the standard of reference, the figures are sufficient to confirm the conclusions to which the argument which I have followed in this lecture has led.

LECTURE IV.—Delivered April 15, 1889.

In any of the combinations of thermopile and galvanometer described in the last lecture the immediate effect of a current is the production of a twisting force or moment between the coils and needle, which produces a deflection in the moveable part of the apparatus, that is, in the needle. If the needle were fixed, and the galvanometer coil moveable, then the coil would be turned with the same force as the needle was before, but such a construction of apparatus would be impracticable. Supposing, however, an instrument with a fixed needle and a moveable coil to be made, the deflecting force would be simply proportional to the strength of the magnetic needle, and therefore if, instead of a small needle, a powerful horse-shoe magnet were made use of, then just as the magnetic field in which the coil lies, due to the strong magnet, is stronger than the corresponding field due to the needle, so in the same proportion would the coil be more powerfully deflected for any current by the magnet than it would by the needle. The advantage of the increased field thus available is so enormous that the coil may to a great extent be reduced in size and complexity, and still leave a balance in favour of the combination of magnet and moveable coil.

This was first effected in 1836 by Sturgeon, to whose researches in this direction Professor S. P. Thompson has directed my attention. They are published with his other work in a thick quarto volume.*

Sturgeon used a variety of pairs of metals, which he generally combined by soldering to the extremities of a semicircular piece of wire made of one metal the ends of a straight piece made of the other metal. He then suspended these frames in front of one pole of a strong magnet, and heated one junction. The frames were deflected one way or the other, according to the junction that was heated or the pole of the magnet that was employed. I have made one of Sturgeon's frames, modified in detail only, in conformity with our modern knowledge, and I shall be able to show that this very simple contrivance is capable of showing very small effects of heat. It consists simply of a rectangular frame, of which the upper side and the two ends are made of copper wire, while the lower side is composed of two bars made of the alloys described in the last lecture, soldered end to end. A needle point is soldered to the middle of the upper side, which rests upon a piece of glass, so as to allow the frame to turn freely. The two poles of a strong horse-shoe magnet are thrust through the open frame without touching it anywhere, and an index of straw moving over a scale shows the deflection of the frame, and the direction in which it moves. A small piece of iron wire attached to the frame serves to bring it to the zero of the scale. If now a lighted match is held opposite to the centre of the compound bar, the frame almost immediately begins to move in one direction, while if heat is applied to the ends of the compound bar, it instantly swings round and is deflected in the opposite direction. I may perhaps here remark that Sturgeon found that a coil suspended in a magnetic field made a most delicate galvanometer.

M. D'Arsenval has invented an instrument on this principle, which he showed at a meeting of the Physical Society of France, on February 5th, 1886. It consists simply of a pair of wires, one of silver, and the other of palladium, soldered together at their extremities, and forming a rectangular frame, with the junctions in the middle of the upper and lower sides. The frame is suspended by a single fibre of silk between the poles of a horse-shoe magnet, and is directed by a fragment of iron wire attached to it. He has made the frame of two different forms, either short and wide, in which case he pla_es within

* "Scientific Researches, experimental and theoretical, in Electricity, Magnetism, Galvanism, Electro-Magnetism, and Electro-Chemistry," by William Sturgeon.

it a fixed cylinder of soft iron, to increase the strength of the field in which the circuit hangs, or else very long and narrow when there is no room for an iron core; but in this case the legs of the magnet can be made so close together as to attain the same object. The deflection is read by means of a mirror in the usual way, but in this instrument the mirror serves a double purpose, screening, in addition, one of the junctions over which it is fixed from the influence of any radiation that might fall upon it as well as upon the other junction, but this is a point to which I must refer later.

M. D'Arsenval has found that the instrument is very sensitive, and in the case of the long and narrow frame, that it is also exceedingly rapid in its movements and dead beat.

About a year after M. D'Arsenval had published an account of his instrument, I heard of the results obtained by Langley with his bolometer, an instrument which, in his hands, became far more delicate than the ordinary thermopile. I felt that it was very unfair to the thermopile to compare it in its ordinary form, as made thirty years ago, with Prof. Langley's instrument, in the development of which all the knowledge and resources of the present day have been made use of. Owing to the very high electromotive force set up at a thermo-electric junction, and the very small temperature co-efficient of resistance of conductors, on which the bolometer depends, I fancied that if an instrument depending on thermo electromotive force were designed and carried out as well as the bolometer had been, that perhaps a still more delicate and satisfactory instrument might be the result. It was in trying to solve this problem that I devised an instrument the same as Sturgeon's and M. D'Arsenval's in principle. Had I been aware that M D'Arsenval had designed an instrument of the kind, I should almost certainly have thought no more about it. It is perhaps well that I was ignorant of the work of this distinguished *savant*, for not only have I developed the theory of instruments of this class to a considerable extent, but I have made an instrument which I am sure is far more sensitive than his, and which at the same time must be less affected by numerous disturbing causes than any other instrument for measuring radiant heat that has been made, and, in addition to this, it was owing to the difficulties that I met with in trying to find a suitable suspending fibre that I was led to the process for making fine fibres of quartz.

In my instrument, which, at General Donnelly's suggestion, I called a radio-micrometer, a circuit is suspended in a magnetic field. The circuit is composed of three metals, as follows:—There are, in the first place, two very small bars of antimony and bismuth, or, preferably, of the alloys to which I have so often referred, which are soldered side by side at their lower ends to the side of a small disc, or, for spectrum work, to the end of a narrow strip of copper foil, on which the radiation is to fall, as in Lord Rosse's thermo-junction, while their upper ends are soldered to the ends of a long, narrow ∩-shaped piece of copper-wire, which completes the circuit. The upper end of the copper stirrup has soldered to it a small piece of straight wire, which is cemented into the end of a very fine glass tube. At the upper end of the glass tube is fixed a small plain mirror, and the whole is suspended by a fine quartz fibre in a narrow hole in a mass of brass, or better of copper, between the pole pieces of a powerful magnet.

I have investigated* the theory of this instrument with the view of obtaining the best possible result. The main conclusions are, I think, of sufficient interest to bring before your notice.

In the first place, it follows, for reasons similar to those advanced in the discussion of the thermopile and galvanometer, that the dimensions of the moving system should be as small as it can be made, provided that whatever size it is made the several parts are properly proportioned. The limit of smallness is practically determined by the bars of antimony and bismuth, or of alloy, because of the difficulty of making and soldering such materials when excessively thin. I find by experience that I have no difficulty in making these bars far finer than any which at first I expected it would be possible to handle, and in soldering them at each end, and even, if necessary, taking a circuit to pieces, cleaning off the solder and re-soldering, when they are no more than one-two-hundredth of an inch thick, and one-fiftieth of an inch wide, that is so fine that thirty of them could be packed in the space occupied by a single bar, such as is used in an ordinary thermo-electric pile of eighty pairs.

Now, whatever size is given to the bars, it is clear that for any size of bars there is some thickness of copper wire which is most suitable, for if it is made very thin indeed, the conductivity of the suspended system will be reduced in a higher ratio than the moment of inertia;

* "Phil. Trans." Vol. 180, 1889, p. 159.

on the other hand, if very thick, the moment of inertia will be increased in a higher ratio than the conductivity. But, what is not immediately evident, is that no matter what shape or size, or number of turns may be given to the copper wire, one particular thickness is better than any other. This is given by the relation—

$$a = \frac{1}{b} \sqrt{\frac{Kv}{Cu}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Where a = the sectional area of the copper wire.
b = the breadth of the circuit (assumed small compared with the length).
u = one quarter of the density* of copper.
v = the specific resistance of copper.
K = the moment of inertia of the bars, mirror, and connecting tube.
C = the resistance of the bars.

There must also be some size of circuit which will give the best result, that is the greatest deflection in a given field, and with a given period of oscillation, for if made very large or very small, more is lost than gained. The best area for the circuit to enclose is given by the equation—

$$A = lb = \tfrac{1}{2} \sqrt{\frac{KC}{uv}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Where the symbols have the meanings already given, and in addition—

A = the area enclosed by circuit.
l = the length of circuit.

In all cases one turn of copper wire is better than any greater number. It does not matter how l and b are modified so long as b is kept small compared with l, and so long as their product is not allowed to change. Under these conditions the area enclosed by the circuit, the moment of inertia, and the resistance of the circuit all remain the same, and therefore the sensibility is not affected.

When a and A have the values given by the two equations above, the following very simple relation will be found to hold. The resistance of the copper part of the circuit is equal to the resistance of the bars and the moment of inertia of this copper is equal to the moment of inertia of the bars, the mirror, and the connecting stem. Further, what I have called the efficacy of the combination, that is the sensibility in a unit magnetic field, is—

$$E = \tfrac{1}{8} \frac{1}{\sqrt{(KCuv)}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Though this is the circuit which will give the greatest deflection in any field when mounted so as to have any given period, it does not follow that it will be the most convenient one to use in a very strong field. The force tending to move the circuit, and the ultimate deflection, is proportional to the strength of the field, but the resistance to the motion due to the reaction between the field and the current induced by the motion is, for any speed, proportional to the square of the strength of the field; and so, though with weak fields the circuit may move readily enough, this is not the case when the field is strong, even though the force urging it to move is greater. I can show the effect of this "damping" by a simple experiment. There is suspended between the poles of a magnet a circuit made of copper only, and the strength of the field in which it hangs may be varied by moving the pole pieces of the magnet. At present the field hardly exists, and so the copper circuit is able to oscillate freely, and in consequence to make many swings before it comes to rest. On increasing the strength of the field, it is evident that the circuit is not so free as before, because any swing is only a small fraction of the one before it, and after four or five oscillations it ceases to move. On further increasing the strength of the field, the oscillations fall in amplitude at a still higher rate, and they seem each to take very little longer, but there are fewer of them before the visible movement comes to an end; at last the resistance to the motion become so great that the circuit when displaced moves up to its position of rest, and is unable to pass beyond, but now it takes an appreciably longer time to make the half swing than it did before. On still further increasing the field, the resistance becomes so great that the circuit is hardly able to move at all, but very slowly creeps along, and may take ten, or perhaps a hundred times as long to come to its resting place as it did in the last case.

The question that then arises is what is the most suitable field to employ; if it is weak, the circuit will oscillate so freely that owing to the number of swings it may take a long time to come to rest, and further, the deflection will not be great. If the field is as strong as it can be made the circuit may meet with so much resistance to its motion that it will take an enormous time to come to rest, though it is true the deflection, when it can be read, will be much greater. It will make it easier to come to a just conclusion, if I state in a few cases to

* That is the moment of inertia of unit volume when the circuit has unit breadth.

what extent the period of oscillation is increased when the resistance to the motion is sufficient to produce certain definite decrements in the amplitude of the oscillation.

	Ratio of any Oscillation to the one before.	Period of Oscillation.
Undamped	1	1·00
	$\frac{1}{2}$	1·02
	$\frac{1}{5}$	1·12
	$\frac{1}{10}$	1·24
	$\frac{1}{100}$	1·77
	$\frac{1}{200}$	1·96
	$\frac{1}{1000}$	2·42
	$\frac{1}{10000}$	3·10
	$\frac{1}{100000}$	3·80
	$\frac{1}{1000000}$	4·51
Dead beat	$\frac{1}{\infty}$	∞

Now the strength of the field should be so chosen that the resistance to the motion of the circuit caused by it is not quite sufficient to make the motion perfectly dead beat. Supposing that it is possible to observe a deflection accurately to, say, one-thousandth of the whole, it is not only useless to make the decrement less than $\frac{1}{1000}$, but harm will be done because of the rapidity with which the time of coming to rest is increased when the magnetic field is made stronger. On the whole, I think it is preferable to have the decrement such that the elongation of the first swing beyond the position of rest is just distinguishable as an elongation, for then it is possible to make a definite reading in a very short time, a matter of importance when experiments are being carried on under variable conditions.

However, it is a matter of not much consequence whether the ratio of damping is very small, or whether the motion is just dead beat, what is really important is that the resistance shall not be more than sufficient to make the motion dead beat. If, for instance, the field that is just sufficient for the dead beat conditions is doubled in strength, then though the ultimate deflection obtainable may be also doubled, the velocity of motion will be halved, and it will take four times as long for the circuit to apparently come to rest.

Now when anything, as is the case here, is subject to a force proportional to the displacement, and to a resistance proportional to the velocity, the motion will be just dead beat when half the resistance at unit velocity is equal to the square root of the controlling force at unit displacement. It may be proved that when the circuit is made of the dimensions which equations (1) and (2) show to be best, the value of the magnetic field, H, that will just make the motion dead beat is given by the equation—

$$H = 8\sqrt{\frac{\pi}{\tau}}\ \sqrt{(u\,v)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

in which τ is the complete period of vibration (undamped).

Since the symbols K and C have been eliminated, this shows that, no matter what the bars are made of, or what dead weight is fastened to them, provided the copper part of the circuit is formed so as to give the greatest efficacy, the magnetic field that will just make the motion dead beat, conveniently called the dead beat magnetic field, will always be the same, and this depends simply on the density and specific resistance of copper, and the period that is chosen.

It follows from equations (3) and (4) that the sensibility, S, obtained by the best circuit in the dead beat magnetic field, may be found from the relation—

$$S = \sqrt{\frac{\pi\,\tau^3}{(K\,C)}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which is independent of the properties of copper. It thus appears that the sensibility obtained by the above combination is not affected by the nature of the material with which the circuit is completed, so that a badly-conducting metal, or even glass, would be as good as copper. This very paradoxical result may be explained by imagining what would happen if a specimen of copper could be found with one hundred times its proper resistance. Under these circumstances, equation (1) shows that the wire would have to be made with ten times the sectional area, and equation (2) that it would be ten times as short, and thus both the resistance and the moment of inertia of the circuit would be the same whichever metal were used. Now equation (4) shows that the field would have to be ten times as strong, from which it immediately follows that the motion must still be dead beat, if it was so before, since the circuit has the same moment of inertia and the same resistance as before, but encloses one-tenth of the space in the magnetic field, which confirms equation (4), and that the sensibility must be unchanged, which confirms equation (5). Of course, practically, glass could not be used to complete the circuit, because with such bars as it is possible to make, the thickness of the glass would become enormously greater than the length of the circuit, which must, by original assumption, be large compared with the

breadth, and because a magnetic field of an almost infinite strength would be necessary.

The actual strength of the dead beat magnetic field, that corresponds to the material copper and the arbitrary period 10 seconds, is almost exactly 272 C.G.S. units. Now, since it is easy to obtain a field four or five times as strong as this between the poles of a permanent magnet, it is a question whether it will be possible to use a much stronger field with advantage, not without varying some of the other conditions, for that would cause a resistance to the motion of the circuit of from 16 to 25 times that which is necessary to make it dead beat, so that it would require from 16 to 25 times as long a time in which to come to rest, but with such a modification in the circuit as will keep the motion dead beat. The thickness of the copper wire must not be altered, but the size of circuit may be reduced as the field is increased in strength, so as to maintain the dead beat relations, and the result is a slight gain in sensibility. Calculation shows that if the field is made M times as strong as the dead beat magnetic field, the area must be made $(2M-1)$ times as small, while the sensibility of the combination will become $\left(2-\frac{1}{M}\right)$ times as great. Thus with a field four times as strong, the sensibility will be $1\frac{3}{4}$ times as great, while with an infinite field it could not be more than doubled.

The practical conclusion, then, is that if a circuit is made approximately of the best proportions, the field may be varied by sliding the pole pieces until a convenient decrement is produced, when the sensibility will not greatly differ from the greatest which it is possible to obtain.

I may mention also that the size of the mirror is a matter of some importance. If large, so as to give plenty of light, say as large as the mirror of an ordinary reflecting galvanometer, the moment of inertia would be so enormous as to completely spoil the instrument. If very small, so as to have a negligible moment of inertia, it would neither reflect enough light nor would it on optical grounds be capable of defining sufficiently well. That size is best of which the moment of inertia is about one-third that of the bars. I found that mirrors made of the thinnest microscopic cover glass, one two-hundredth of an inch thick, and about one-eighth of an inch square, silvered at the back, fulfilled these conditions in the case of the particular circuit that I have in the instrument upon the table. I have found that the definition of such a mirror, if properly made and mounted, is so good that it will produce an image of a cross wire upon a scale one metre distant, which is a sharp black line not much more than one-tenth of a millimetre wide, and which can certainly be read to this degree of accuracy. I have not found that galvanometers are usually read more accurately. It is, of course, necessary in the case of these small mirrors to employ a brighter light than a lamp flame, but, with oxygen at its present low price, there is no reason why a small limelight should not be used. The filament of an incandescent lamp burning brightly also answers well.

The following dimensions, which are nearly those given by the equations, I have found by experience to answer well, and to be not so small as to be too difficult to make:—Thermoelectric bars $\frac{1}{3} \times \frac{1}{50} \times \frac{1}{200}$ inch. No. 36 copper wire made into a circuit one inch long, and about $\frac{1}{10}$ inch wide, a copper heat-receiving surface, blackened on the side exposed to the radiation $\frac{1}{10}$ inch in diameter, or $\frac{1}{4} \times \frac{1}{30}$ inch. Mirror $\frac{1}{10}$ inch square, $\frac{1}{200}$ inch thick. Quartz fibre 4 inches long, $\frac{1}{6000}$ inch in diameter.

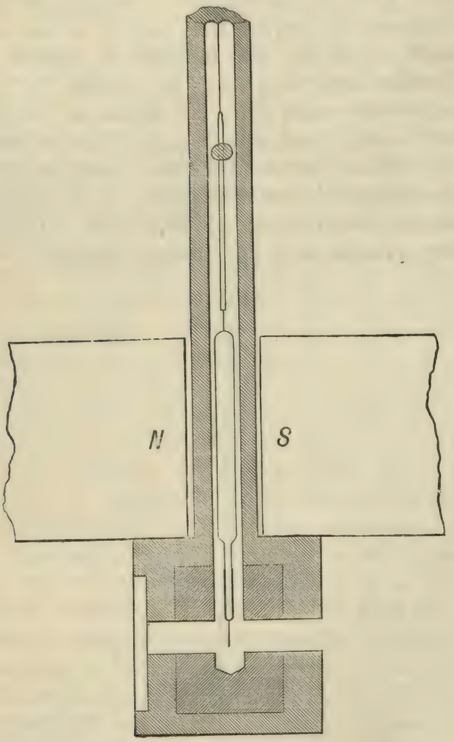
FIG. 10.

The complete circuit connecting stem and mirror, *m*, is shown in Fig. 10. One of these circuits that I made weighed less than half a grain, and though there were five soldered joints, the total weight of solder used did not exceed $\frac{1}{65}$ grain.

There is one point that I should mention.

The disturbance caused by the magnetic qualities of the antimony and bismuth bars, small though they are, was so great as to make the instrument completely unusable; but this difficulty was overcome by making the centre of the block of metal in which they hang of iron, as shown by the darker shading in Fig. 11. This screens off the magnetism of the pole-pieces from the bars, but leaves the rest of the circuit in a strong field. The copper wire used must be carefully chosen, as much is so magnetic as to be useless; when a non-magnetic piece is found, it must not be cleaned with emery, or it will become evidently magnetic. I mention this to show how feeble the forces

FIG. 11.



are that are used, and when I say that the instrument is perfectly free from every influence except that of radiation upon the receiving surface, it will be evident that the effects of many disturbing causes which ordinarily give so much trouble have been very completely avoided. A strong magnet may be moved about close to the instrument, but no effect whatever can be observed. Magnetic disturbances are the most fruitful source of trouble with the ordinary galvanometer; for instance, it is not possible to do any serious work with a galvanometer in the Science Schools, South Kensington, except at night, because of the movements of an hydraulic lift, of which the ram is a huge weak magnet, presenting in its movements alternately north and south poles to every instrument on the ground floor. There are no connecting wires or binding screws, and so no uncertain thermo-currents are set up, nor induced currents due to the movement of the connecting wires through the earth's magnetic field. The sensitive part of the instrument is very small, and is enclosed in a narrow hole in a solid mass of metal, which, moreover, is protected by being enclosed in a wooden case, and so temperature changes in the room, and hot and cold draughts are not felt. The instrument is very quick in its indications, its sensibility and its decrement can be varied at will. The following figures, obtained

	Deflection.	Deflection. / Heat.	Deflection. / Heat × time.
Elliot pile & galvanometer	1	1	1
Radio-micrometer	4·2	560	2,000

by experiment from the instrument now on the table, show to what extent it is more sensitive than the standard thermopile and galvanometer, as measured in the three ways described in the last lecture. This sensibility has not been obtained at the sacrifice of stability, for none of the other instruments will compare with this in its freedom from the influence of every kind of disturbing cause.

If it is desired to find out what temperature difference between the ends of the bars will be required to produce a given deflection in an instrument in which the circuit of greatest efficacy and the dead-beat magnetic field are employed, the following equation will give the result:—

$$\text{Temperature difference} = \frac{16\,\alpha'}{3\,\theta}\sqrt{\frac{\pi^3}{\tau^3}}\,\sqrt{(KC)} \quad ..(6)$$

Where α' is the angle of deflection, θ the thermo-electric power, and the other quantities are as before.

If α' is supposed to be $\frac{1}{10000}$, that is a deflection of $\frac{1}{5}$ mm. on a scale at a distance of one meter, if θ is 10,000, which is cerrainly too low a value, aud τ is 10 seconds, then the temperature difference that would be necessary to produce the deflection α would be $5·52 \times 10^{-7}$ degrees Centigrade. If it is supposed that the magnetic field is made four times as strong as that which just makes the motion dead beat, then the best circuit must be reduced in area until the motion is again just dead beat; the sensibility will then be $(2^{-\frac{1}{4}})$ times as great, or the difference of temperature

necessary to produce any deflection will be $(2 - \frac{1}{4})$ times as small, so that $2 \cdot 58 \times 10^{-7}$ degrees will produce the deflection supposed. Thus about a four-millionth of a degree should produce a deflection of one-fifth of a millimetre, or one millionth of a degree of nearly one millimetre.

As the value 10,000, assigned to θ, is about that of antimony and bismuth, and the alloys are one-third greater, it is probable that an instrument as well made as possible would, with a period of ten seconds, give a deflection of about one millimetre for every millionth of a degree, by which the lower ends of the bars are heated above the upper ends.

Experiment shows that the instrument on the table will clearly respond to a quantity of heat no greater than that which would be radiated on to a halfpenny by a candle-flame 1,530 feet away from it.

There is one other class of instrument for measuring radiant energy, which has lately been brought to great perfection more especially by Prof. Langley. Instruments of this class depend upon the change of resistance of a conductor when warmed. The earliest account of an instrument of this kind, for the reference to which I have to thank Dr. Baur, is one by A. F. Svanburg,* who made one of the arms of a Wheatstone's bridge, of a flat spiral of copper wire $\frac{1}{120}$th of an inch thick, covered with lamp black. When this spiral was exposed to radiation, it was warmed to a certain extent, and so its resistance was changed, disturbing the balance of the bridge. He found this very simple contrivance to be extraordinarily sensitive, and better, he believed, than the Nobili thermopile.

It does not appear that much use had been made of instruments depending upon the effect of temperature upon resistance until 1881, when Langley turned his attention to this class of instrument. However, Jamin and Siemens had contrived apparatus in which the change of resistance disturbed the equilibrium of a differential galvanometer.

The general theory of the actinic balance or bolometer is given in the *American Journal of Science*, vol. xxi., p. 187. From this paper it appears that Langley did not find the thermopile sufficiently delicate to detect and measure the energy in a diffraction spectrum. In the case of the thermopile and galvanometer, the work that is necessary to deflect the needle must be supplied by the energy of the radiation, in fact, a great deal more must be supplied, because nearly all the heat received by the pile or junction which is not conducted away is carried by the Peltier action of the current to the cooler junction, and but a small fraction is converted into electrical energy, only part of which is transferred to the needle. In the form of instrument first used by Svanburg, the energy that is necessary to move the needle is not derived from the radiation at all, but from the cell or battery employed to send the current through the Wheatstone bridge. All that the energy of the radiation has to do is to direct that of the battery, for when one of the resistances in the bridge is increased, and the balance disturbed, some of the energy of the battery spends itself in the galvanometer. The materials which Langley found most suitable for the resistances were steel, platinum, or palladium, in the form of the thinnest possible ribbon. In the case of iron, the change of resistance is 0·4 per cent. for each degree Centigrade. The object of using ribbon as thin as it can be made is to cause it to come to its final temperature as quickly as possible.

The effect of the size of the ribbon is not at first very evident. Suppose a bolometer made with a strip of metal of some particular size, and that another identical instrument is made, in which the strip that is to be exposed is half the length and half the breadth. Then its resistance will be the same; and since in the smaller strip there is only one quarter of the metal to heat, and it exposes one quarter of the surface to the radiation, it might seem as if it should acquire the same temperature as the larger piece, and so the change of resistance in each case should be the same, and thus the smaller piece receiving the smaller quantity of heat seems as if it should be able to produce the same effect as the larger. Now there are several false assumptions in this reasoning. In the first place, when these strips of metal are warmed, however slightly, above the rest of the instrument, heat begins to escape by radiation, convection, and by conduction. The radiation would, for any rise of temperature, be itself proportional to the surface, and so would produce the same effect in the two cases; the same would be almost true of the heat lost by convection; but this would not be the case at all with the loss by conduction, so that on this account the smaller surface would not be heated to so great an extent as the larger. However, even supposing that they were heated to the same extent, when no current passed, the

* "Pogg. Ann." 24, p. 416, 1851.

smaller piece would not be capable of causing so great a movement of the galvanometer needle. It must be remembered that the battery current itself, in passing through the several resistances, must heat them to a certain extent; but as the exposed strip is electrically balanced against another strip in the same tube, but not exposed, this alone does not disturb the balance, unless the current is so strong as to heat them sufficiently to set up strong convection currents in the apparatus. Now in the case of the larger strip, a stronger current can be sent than through the smaller before this occurs, and since they are of the same resistance, a stronger current in the galvanometer will be the result.

In another way the heat produced by tha battery current and that developed by the radiation to which the instrument is exposed, are curiously involved together. Suppose that the radiation acting alone were able to heat the exposed strip so as to increase its resistance to a certain extent, and the current acting alone were able to heat both strips to some other extent (generally from a thousand to a hundred thousand times as much), then when both act together there will not be the same difference of temperature as if the battery current were not passing, and for this reason. Any increase of temperature in the exposed strip, by increasing its sistance, diverts a certain proportion of battery current into the other strip, and thus more heat is developed in the covered strip by the battery current than in the exposed strip, and this tends to counteract the effect of radiation. Thus apparently heat is carried from the warmer to the cooler strip, just as in the thermo-electric apparatus the Peltier action carries heat from the warm to the cool junction. An exception to this, however, is found in the case of carbon, which falls in resistance as its temperature rises. In this case the heat received by the exposed strip warms it, lowers its resistance, and causes a greater proportion of the whole current to pass through it, which warms it still more.

Langley has, by making every detail as perfect as possible, and by employing the most delicate galvanometer that American ingenuity could construct, been able to map the dark heat* of the spectrum, and to extend it far beyond the limits which previously were known.

Dr. Baur has published two papers† on the bolometer. He made his grating of tinfoil, cut in the form of a series of parallel strips joined at alternate ends (a form which Langley also used) supported at the ends of the strips only, and blackened with platinic chloride. Such a sensitive surface acquires its final temperature almost instantly, and the time that elapses before a reading can be taken depends simply upon the galvanometer. Dr. Baur tried to use Dutch gold and gilt paper, but these were found impracticable. There is a difference in detail between the arrangements of Langley and Baur in respect of the second resistance, against which the exposed surface is balanced. In Langley's instrument this second resistance is made in two halves, placed on either side of the exposed surface so as normally to have the same temperature. In Baur's arrangement the two resistances are arranged side by side, and by the movement of a shutter the radiation is allowed to fall on one or the other alternately, and thus the effect is doubled. In order that these two resistances should be exactly alike, a piece of foil was doubled, and the two cut out of the doubled piece at the same time.

I understand from Dr. Baur that this class of instrument is in use in the laboratory of Professor Helmholtz, and generally in Germany to a much greater extent than it is with us.

I have had no experience with any of the instruments of the bolometer type, and so cannot speak of them from experience; but it is possible that in sensibility Prof. Langley's instrument may not be far short, if it does not actually exceed, that of the radiomicrometer. But it cannot compare with the radiomicrometer in its freedom from disturbing influences. On the other hand, the bolometer has the very great advantage over all the instruments except the thermopile, that it can be moved about and pointed up or down, whereas the radiomicrometer must be kept level, and is most easily used when fixed, so that the radiation must be brought to it, a plan which in some cases is not convenient.

A few words on the relative advantages of the different classes of instruments may perhaps be conveniently given here.

It sometimes happens that the radiation to be measured is brought to an exact focus, which is a line in the case of a spectrum, or a point when a star is being observed. In these cases, instruments like Joule's convection apparatus, the differential air thermometer, or Weber's micro-radiometer are useless, since

* "Am. Journ. Sci.," xxv., p. 169; xxvii., p. 169; xxxi., p. 1.

† "Proc. Berlin Phys. Soc.," March 3, 1882. "Annelen der Ph. und Ch.," vol. xix., p. 12, 1883.

they are only efficient when advantage is taken of the large surface they expose to the radiant energy. The receiving surface should be no larger than the image formed, and so even the thermopile itself is practically of little use. A thermo-junction is good, but the bolometer for spectra, and the radio-micrometer generally, are the only instruments that can be used with advantage.

If diffused heat is to be measured, then the instruments first mentioned are at their best, but those with small receiving surfaces are better if reflecting mirrors or rock-salt lenses can be employed to concentrate the rays upon the small surface.

If the instrument has to be freely movable, the thermopile and bolometer are the only ones which can be used at all; if it need be moved only slowly, and may be kept level, then the radio-micrometer and one or two others also become available.

If the instrument is to be exposed to outside changes of temperature, the radio-micrometer is the only one which is practically uninfluenced.

If magnetic changes, which are by no means uncommon, are liable to be met with, the radio-micrometer and Joule's instrument are the only ones available.

With regard to my instrument, and M. D'Arsenval's, I have no doubt that mine is far the most sensitive and the least influenced by disturbing causes; for I employ a thermo element which has an electromotive force ten times as great as any that he can make use of. He is able to suspend his circuit by a fibre of silk, which would make the radio-micrometer practically unmanageable. He has not, so far as I know, screened off the disturbing effects of temperature changes by surrounding the circuit with a mass of metal, and if this were done, since the junctions are much further apart in his instrument than in mine, any temperature waves moving in the metal block would have a greater effect; but what to me is most conclusive of all, is the fact that he fixes the mirror over the junction that is not to be exposed to radiation, and so leaves it practically unprotected instead of being, as in the radio-micrometer, deeply buried in the centre of the instrument out of reach of any radiated heat.

And now, in conclusion, I have to regret that it has not been in my power to treat the subject of these lectures either so clearly or so thoroughly as I should have liked. The only claim that I have to address the Society of Arts at all upon the subject is the fact that I have done something to develop some of the instruments which I have brought before your notice.

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